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BRAITHWAITE'S RETROSPECT.

VOL. LXXIX, JANUARY—JUNE, 1879.

THE
RETROSPECT OF MEDICINE:

BEING

A HALF-YEARLY JOURNAL

CONTAINING A RETROSPECTIVE VIEW OF EVERY DISCOVERY AND
PRACTICAL IMPROVEMENT IN THE MEDICAL SCIENCES.

EDITED BY

W. BRAITHWAITE, M.D.

LATE LECTURER ON MIDWIFERY AND THE DISEASES OF WOMEN AND CHILDREN
AT THE LEEDS SCHOOL OF MEDICINE, ETC.

AND

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SYNOPSIS,

(ARRANGED ALPHABETICALLY), CONTAINING

A SHORT ABSTRACT OF THE MOST PRACTICAL ARTICLES IN THIS VOLUME, SHOWING
AT A GLANCE THE MOST IMPORTANT INDICATIONS OF TREATMENT PUBLISHED
BY DIFFERENT WRITERS WITHIN THE HALF-YEAR.

AFFECTIONS OF THE SYSTEM GENERALLY.

ACUTE RHEUMATISM.—Painting the affected joints in the acute continued form with a mixture of equal parts linimentum iodi and tincture of iodine has appeared to me to procure all the benefit advocated by Dr. Davies for his blister treatment, without producing the unnecessary pain of open blister wounds, or risking the production of acute nephritis or cystitis. The linimentum alone is for most skins too strong; the tincture of our Pharmacopœia for nearly all useful purposes too weak. I am no advocate for, and have never allowed, my patients to be placed between the blankets instead of the sheets; neither will I permit flannel bed-shirts to be worn. The object I have in view is to keep one uniform temperature about them, which the bed provides for, but not to keep the patient swathed in flannel reeking and saturated with decomposing perspiration. As a matter of fact, I observed that a large proportion of hospital rheumatic patients were persons who habitually wore thick flannel shirts and undershirts, and did not change them very often. Cotton shirts and cotton sheetings are better suited than linen to rheumatic people. My practice has been to keep my rheumatic cases cool rather than hot; they are certainly easier in consequence. As to baths, a tepid wash-bath at 98° or 96°, into which they can be dropped in a sheet and lifted similarly back into bed, comforts them and secures their cleanliness. The alkaline treatment or peppermint water is equally well adapted for the acute continued forms; but it appears to me that alkalies, unless pushed to the full and frequent doses first urged by their original proposers, are wholly useless in relieving pain, while, when they are given in half-drachm or drachm doses of bicarbonate of potash every two or three hours, they quickly reduce the strength and render the patient extremely anæmic. They appeared to me, thus employed, to diminish, perhaps prevent pain, but to retard convalescence not inconsiderably.

Salicylate of soda appears to lower the pulse, to diminish the temperature, and to relieve the articular pains; but to secure these effects it has to be given in moderately large and frequent doses. I usually give 15-grain doses of it every two hours until 60 grains have been taken, and then keep up the effect of the drug with 15-grain doses every four hours. Quinine and iodide of potassium has long been a favourite draught of mine in treating the anæmic relapsing forms. Two grains of quinine are combined in it with five of iodide of potash; and this draught I generally order to be taken after the 14th day every four or six hours. (Dr. R. Southey, p. 33.)

DIPHTHERIA.—*Sulphur as a Topical Application.*—Procure a quill, and through it blow upon the tonsils some sulphur. This can be repeated at intervals by one of the attendants of the patient. (Dr. J. A. Erskine Stuart, p. 26.)

[Our own plan of treating a diphtheritic throat is very simple, and very rarely indeed fails. We tear out and remove by friction with a morsel of cotton wool, the diphtheritic exudation. The wool is wrapped up rather tightly and held in a pair of strong and slightly curved forceps. It may be dipped in a solution of nitrate of silver, or in sulphurous acid. It is not, however, the application which is the active agent, but the friction, which should be from behind forwards, attended with some rotation of the morsel of wool. At the same time we give internally a mixture containing iron, glycerine, and chlorate of potash.—EDS.]

FEVERS, SMALL-POX, SCARLATINA, MEASLES, HOOPING-COUGH.—*Periods of Incubation.*—The period of incubation of small-pox is twelve days when the poison enters the system in the ordinary way, but only seven when introduced by inoculation. The incubation period of measles is ten or eleven days, to the appearance of the primary fever. That of hooping-cough is probably under a fortnight; that of typhus twelve days; that of enteric fever a fortnight, but in some instances a little longer, but never beyond three weeks. The period of incubation of scarlet fever is the shortest of all, and varies from one to five days, but in rare instances there is no incubation period at all. (Dr. C. Murchison, p. 1.)

GOUT AND RHEUMATISM.—*Omission of Soda Salts and Alcohol.*—Dr. Drew relates a case in which abstaining from alcohol cured the gout, but the stiffness of the joints remained. The effect of leaving off salt was remarkable. After waiting patiently for all that recuperation which the *vis insita nature* was able to accomplish, there still remained the

stiffness of the hands and the periosteal thickening of the phalanges, which nature, unassisted, could not entirely overcome. Suddenly, about five weeks ago, whilst eating a morsel bountifully seasoned with salt, the thought occurred, "Can chloride of sodium be right for a gouty system, when cakes or biscuits or tea made with soda, or even soda-water (when containing that alkali), will produce an attack of this disease?" Certainly not, theoretically; for an atom of oxygen united with the sodium will present to the uric acid of the blood the worst poison it could possibly encounter. The idea once started, it was, of course, immediately carried into practice, and chloride of sodium was placed in the *Index Expurgatorius*. Salt was omitted as an article of diet, not merely as a condiment, but avoided in salted meat or any other suspected comestible. The result in four or five weeks has been astonishing: most of the stiffness has passed away; rings that had been laid aside can be reworn; and the phalangeal bones have almost returned to their primitive size and shape. (Dr. J. Drew, p. 37.)

SCARLATINA.—Basing his opinions upon more than 400 cases of scarlatina which he treated during six consecutive years, the author observes that he is unable to see any relation as cause and effect between the disease and bad drainage, except in so far as the latter, by polluting the atmosphere, and thereby deteriorating the health of individuals exposed to it, renders them less able to resist the influence of zymotic and other similar poisons. With enteric fever, on the other hand, we have strong evidence tending to prove that the disease is propagated by the drinking of water contaminated with sewage matter containing the elements of the disease, though every now and then cases occur which tend to support the theory of its contagiousness. But in the case of scarlet fever, small-pox, measles, and the like, it appears to me that overcrowding is a far more powerful cause of the spreading of those diseases than any other; and that regular and periodical inspection of the houses of the poor with the object of checking the same is of as much importance as effectual drainage, valuable as that undoubtedly is. From a careful study of the map and of the above statistics we have arrived at the following conclusions, viz.:—That scarlatina when prevalent is pretty equally distributed all over the town; that defective drainage has little or nothing to do with its spreading, for we find that it was equally prevalent in localities recently and deeply drained and in those where old and superficial drains existed; that the modern and improved drainage has not expelled it from places where it previously had its habitat;

and in several localities where the drainage was known to be most defective, none or but few cases occurred. This, I am informed, is also in accordance with the opinion of Dr. Buchanan, who visited the town (Northampton) after the occurrence of the epidemic in 1870. To prevent the disease spreading when once it has entered a house, complete isolation of the patient is absolutely necessary, and free and systematic use of disinfectants should be insisted upon. An excellent plan is to nail a sheet outside the room door and keep it thoroughly wetted all over with carbolic acid. The simplest and most effectual means of disinfecting the furniture of the room, articles of clothing, &c., is by means of the fumes of sulphurous acid disengaged by burning sulphur. (Dr. C. J. Evans, p. 5.)

SORE THROAT.—*Diagnosis of.*—To me it appears impossible to be sure, on first seeing a case of sore-throat, whether it will be diphtheria or not, even indeed if there should be false membrane. It is now almost certain that some low organism has an influence over the origin and propagation of true diphtheria, and it is very probable that these organisms only find a suitable soil in lowly vitalised or perhaps decayed animal matter, in a manner similar to the mycelium in the thrush of infants enfeebled by illness or want of care. So, in local inflammations, especially of the throat (whether from cold, chill, or other cause), the organism of diphtheria may readily find a suitable soil in the thrown-off epithelium and decomposing exudations of the inflamed mucous membrane. We should, therefore, be careful and tentative in the opinion we give on first seeing a case of sore-throat. Still, I am full of hope that if we all steadily set to work to observe carefully, to define our cases clearly, and at the same time, bring into prominent relief all the attendant circumstances, we shall not be long before we bring order out of chaos add another claim to medicine as one of the exact sciences, and so enable it more adequately to fulfil its mission of rendering aid to suffering humanity. (Dr. R. L. Bowles, p. 27.)

AFFECTIONS OF THE NERVOUS SYSTEM.

ALCOHOLIC COMA. — *Diagnosis of.* — In apoplexy, the temperature of the body is above, in drunkenness below, the natural standard of 98° of Fahrenheit's scale, and upon this statement Dr. Richardson founds a distinguishing test. There can be nothing more certain than a thermic test, as it supplies physical evidence, and is easily obtained. It has been well established that alcohol in excess reduces the

animal temperature, and Richardson states that, as the fourth stage of alcoholism is approached, the decline in temperature becomes actually dangerous, and that in man the fall is often $2\frac{1}{2}^{\circ}$ to 3° . It may be concluded that the temperature in man, during a state of alcoholic coma, has a latitude of five degrees; ranging from what may be considered normal down to $93^{\circ}4$. While alcoholic coma has a variable temperature, and, therefore, a thermic test cannot be absolute, yet, the greater majority of cases exhibit a marked depreciation in temperature. An insensible condition proceeding from alcohol, fracture of the skull, opium poisoning, and apoplexy, are all of them subject, at times, to exhibit low temperatures, and, therefore, the thermic test *cannot* be relied on as a means of differential diagnosis between these various conditions. When first I had to deal with alcoholic coma, the text books invariably taught that the pupils in that state were dilated, whereas one case after another came under my observation with contracted pupils, and as time went on, it became evident that this was the rule. The pupil is contracted when the person is left undisturbed, and it is dilated when an attempt is made to rouse him. It is this order of succession which makes it characteristic, I may almost say, pathognomonic of alcoholic coma. Given an insensible person, who has lain undisturbed from ten to thirty minutes, who then presents contracted pupils, which dilate on the application of external stimulation (without in any way rousing the patient), and which, if the person is left undisturbed, begin to contract again within a short time, and I know of no other state to which this applies than alcoholic coma. It seems as if on the application of external stimulation, though the personal consciousness refuses to return, that the pupil half wakes up and then slowly drops to sleep again. It is nearer the state of the pupil in natural sleep than under any morbid condition, only, the pupils are more sluggish in their movements, and respond feebly to light and shade. The very process of shaking the person, necessary in this test, would at once rouse a sleeper, and so distinguish between sleep and coma, if this indeed were at all necessary. (Dr. W. Macewen, p. 95.)

DELIRIUM TREMENS.—Undoubtedly the most successful plan of treating a case of delirium tremens is to give a sufficient dose of hydrate of chloral to procure a sound sleep. For this purpose half a drachm, repeated again after an hour's interval, usually suffices. In several severely maniacal cases a dose of forty-five grains has been administered, with a result equally gratifying and surprising. In a case of puerperal mania one such dose sufficed to restore reason to the patient, at least so far as quietness and docility were concerned,

though it completely failed to make her believe that she ever had given birth to a child. So far, therefore, as our present experience is concerned, we seem to possess in hydrate of chloral a remedy which in all such cases, from the slightest to the most severe, acts rapidly, safely, and efficaciously—*cito, tuto, et jucunde*—and which seems to deprive indulgence in drink of all its horrors, and nearly all its dangers. (Dr. G. W. Balfour, p. 102.)

Capsicum in Extreme Cases of Delirium Tremens.—In cases of delirium tremens it may be found that half-drachm doses of tincture of capsicum, in water, every four hours will give relief. A good plan also is to give it with brandy and egg. In the second of two cases related by Mr. Crowther the patient fell into a calm sleep after the first dose. Sleep also followed two or three doses in the first case. (Mr. Bingham Crowther, p. 104.)

EPILEPSY WITH OR FROM IRREGULAR AND PAINFUL MENSTRUATION.—In cases of this kind, if the menstrual trouble cannot well be treated, try Leclanche's continuous current, as used by Dr. Myrtle, who says: I commenced with five cells. I passed the current from the lower spine through both ovaries and uterus for fifteen minutes each morning, and gradually increased the strength as she could bear it to twenty cells. She became unwell within a week of her expected day, and continued so for three days. There was no nerve disturbance before or after the period. A few days after she was better, I again applied the current, giving up to thirty cells; and the flow made itself visible the very day on which it ought. In twelve hours, it ceased; so I again had recourse to the battery, and in two days all came right, and the discharge was greater than it ever had been, and continued for four days. She remained perfectly free of all epileptic symptoms, and her temper and health were better than ever. I allowed seven days to pass before applying the current; then I made use of it, going up to forty cells. A week before the expected time, I made her sit daily for fifteen minutes in a hip-bath with a solution of hydrochlorate of ammonia, and passed the current through the water; and on the very day they ought the menses appeared, but again ceased for two days, to reappear and pass through their natural course as on the previous occasion. Again there was perfect freedom from all epileptic signs; so much so that on the second or third day she was out on horseback. Under certain conditions the nerves seem almost insensible to the action of the current; at other times, they resent its mildest application. Another is, that, in giving a bath, care must be

taken that the walls of the bath do not abstract the current from the water and the body of the patient; the best plan being to use a copper wire covered with india-rubber solution for the pole inserted into the water, and for the other any other electrode to be moved over the spine or any part of the trunk of the patient either by her own or attendant's hand. (Dr. A. S. Myrtle, p. 66.)

HABITUAL INJECTION OF MORPHIA.—*Sudden or Gradual Discontinuance of.*—In a case of habitual use of morphia hypodermically, the question as to the best mode of its discontinuance is very important. After relating the case of a lady who succeeded in getting rid of this dreadful habit, the writer observes that a very gradual diminution of the morphia injected is preferable to a sudden discontinuance of it, and seeing that in two years she had got down from fourteen grains a day to three-quarters of a grain, a gradual diminution is proved perfectly practicable. It is, however, the opinion of this lady that there is a greater totality of suffering but spread, of course, over a longer period of time. If, however, from any reason (as formation of abscesses), it is discontinued suddenly, the symptoms resulting will be incessant vomiting and diarrhoea. The vomiting will subside in a variable period, and the sooner, the less the stomach is teased with anything. The diarrhoea, however, will continue for a long period, and is best treated by absolute bodily repose. Neither astringents nor enemata of any kind have any effect in checking it; rather the contrary. An enema of thirty grains of chloral hydrate in an ounce of tepid water at bedtime must be excepted, as this was retained, and proved of service on several occasions. (Dr. James Braithwaite, p. 106.)

HEMIANÆSTHESIA.—In looking upon a woman's nervous system, I often picture to myself an instrument something like a differential thermometer, having a glass globe full of air and a number of pipes leading from it containing fluid, which are constantly influenced by the pressure within. I see the fluid in the pipes unceasingly oscillating or passing up and down under the slightest influence on the bulb, and, in like manner, I see in my imagination a nervous fluid continually in motion, or may be in undulation, through the whole of the nerves of the body. I witness then, as in the girl I mentioned who received a shock to her brain, all undulation cease; in another, cease on one side of the body only. The fright or blow may stop the current, and the limb is for all purposes dead; or it may have an opposite effect, and throw it into a state of jactitation. If these

slight causes will so derange the nervous apparatus, it is not surprising that equally slight causes will set it in order again. The nerve-force seems to be constantly ebbing and flowing under the influence of the most trivial impulses. A fall on a medicine-bottle deprives a girl of movement and feeling on one side, as well as of the use of her special senses, and a sovereign tied on her leg will bring them all back again. I am often reminded of a remarkable watch belonging to a schoolfellow in my younger days, and which, in modern phraseology, was in a state of unstable equilibrium. The mere taking it from his pocket or looking at it would often arrest its movement, when a slight knock would send it on again. The works had never been good, or being out of repair, the slightest jar would stop their action; and, what seemed remarkable, another blow would again start them into movement. So in these women, with their unstable nerves: a violent shock paralyses them, and another shock, be it galvanic or of a cry of alarm, will set them again in action. But not only a sudden effort of the will will set right the machinery, but the application of metals to the limbs or a trephine to the head. I can, therefore, understand how a strong effort of the will will rouse the dormant powers; that is, how the healthy hemisphere can send a stimulus to the inactive one. (Dr. S. Wilks, p. 82.)

AFFECTIONS OF THE CIRCULATORY SYSTEM.

ANÆMIA.—*Arsenic as a Blood and Cardiac Tonic.*—In cases of anæmia approaching in gravity the so-called essential or pernicious anæmia, arsenic is capable of producing great benefit. The dose should commence with three minims, and be increased, if it agrees, to five, three times a day. Of course it should be given after a meal, and may be combined with any other treatment the case may require. (Dr. S. Lockie, p. 123.)

BELLADONNA AS A CARDIAC AND RESPIRATORY STIMULANT.—There is every reason to believe that the antagonistic effect of belladonna in cases of poisoning by opium is due to its influence on the circulatory and respiratory centres. As Dr. Reinhard H. Weber, and others, have remarked, the danger to life in these cases comes principally from the steadily increasing paralysis of the respiratory centres. Hence the great stress which our best authors lay on artificial respiration after all other means have failed. If we can keep up the circulation and respiration, there is a good chance of recovery, especially as the kidneys (excited to action by the

belladonna) are in the meantime constantly removing some of the poison from the blood. From these facts, therefore, taken in connection with the experiments above mentioned, we may conclude that the antagonistic action of belladonna to that of opium depends upon its exerting a contrary influence on the circulatory and respiratory centres to that produced by the latter drug, namely, a restorative and stimulating one. It may also be inferred that belladonna will prove to be equally useful in other poisons which kill in the same manner as opium does. (Editor of Medical Press and Circular, p. 128.)

CARBOLISED CATGUT LIGATURES.—*Action on Human Arteries.*

—The catgut ligature, in its primary effects, acts upon the coats of an artery as any permanent ligature; and as a secondary effect it may and does generally excite an ulcerative action in the external coat of the vessel, even to its complete division. If, therefore, I cannot endorse what the distinguished introducer of the catgut ligature (Prof. Lister) claimed for it in 1869, “That by applying a ligature of animal tissue antiseptically upon an artery, whether tightly or gently, we virtually surround it with a ring of living tissue and strengthen the vessel where we obstruct it”—I may express my belief that, as the loop of the catgut ligature dissolves within an uncertain period, and there is not of necessity any such sloughing or ulceration of the whole coats of the constricted artery as must ensue when a more permanent material is employed, we have in the carbolised catgut the best ligature at our disposal. (Mr. Bryant, p. 247.)

EPISTAXIS, SURGICAL HEMORRHAGES, &c.—*Hot Water as a Hæmostatic.*—At the West London Hospital, having amputated a thigh in the upper third, and there being a great deal of oozing from the stump, especially from a quantity of “nævoid” tissue which the knife had divided, I bathed it with cold water for a long time, but without stopping the bleeding. I suddenly drenched the stump with water of a temperature of 120°. All oozing stopped in a few seconds. In epistaxis, hot water is most effectual when the bleeding comes on during the morning ablution with cold water, or during the progress of a cold in the head. But it answers frequently in cases of traumatic origin. It should be combined with elevation of at least one hand above the head; but the combination is not essential. The water need only be applied freely to the face, need not be injected into the nostrils. The head should not be bowed down over the basin more than is necessary. Upon two occasions when I injected hot water into the nostrils I dissolved a large teaspoonful of chlorate of potash in a tumbler of hot water, and injected the

solution. The saline was used to prevent swelling of the mucous membrane, which so often follows contact with pure water, especially cold water. This hot saline solution stopped the epistaxis instantly. If it be granted that hot water is as effective as, or more effective than cold as an anti-hemorrhagic, then I think it ought to be preferred to cold during surgical operations. (Mr. C. B. Keetley, p. 252.)

INTERNAL HEMORRHAGES.—*Pyrogallic Acid*.—Pyrogallic acid is an excellent internal astringent in cases of hemorrhage. It appears to me to have the following advantages:—The dose is small; it does not disarrange the stomach in the way that the usual gallic or tannic acid mixtures do; it does not cause vomiting, as iron and ergot mixtures sometimes do; it is easily taken, and has no disagreeable after-taste. It appears to be more rapid and certain than any of the remedies mentioned above, and far surpasses the time-honoured acid infusion of roses, or pil. plumbi cum opio. It dissolves readily in water or in spirit. A spirit solution of definite strength affords a convenient and ready method of administration. There is no reason why it should not be also used in the form of spray in hæmoptysis, but I have had no experience of its use in this way. The dose may also be increased according to circumstances. In the Irish Hospital Gazette I recorded some cases of hæmoptysis, &c., in which liq. extract of ergot was used hypodermically with success. A combination of the ergot and pyrogallic acid will afford a very powerful means of arresting internal hemorrhages. (Dr. A. Vesey, p. 133.)

NÆVUS.—*External Application of Sodium Ethylate*.—Sodium ethylate is a crystalline substance obtained by the careful addition of sodium to absolute alcohol. The addition of a small amount of water to this substance transforms it into a caustic. Laid on dry parts of the body the sodium ethylate is comparatively inert, creating no more change than the redness and tingling caused by common alcohol; but as soon as the part to which the substance is applied gives up a little water, the transformation described above occurs, caustic soda is produced in contact with the skin in proportion as water is eliminated, and there proceeds a gradual destruction of tissues, which may be moderated so as hardly to be perceptible, or may be so intensified as to act almost like a cutting instrument. Applied direct to the unbroken skin, their destructive action is less painful than would be expected, and when pain is felt it may be checked quickly by dropping upon the part a little chloroform, which decomposes the alcohol, converting it into a chloride salt and ether. (Dr. J. Brunton, p. 251.)

AFFECTIONS OF THE RESPIRATORY SYSTEM.

ASTHMA.—*Bronchial Asthma.*—I have observed excellent effects follow on the employment of the citrate of caffein. One patient was an eminent medical practitioner in a large town in the north. He had suffered most severely from paroxysmal asthma, and the utter failure of a list of approved inhalations and medicines (far too long to be here enumerated) was most distressing. Four grains of citrate of caffein produced an undue degree of wakefulness, but one grain, taken regularly at bedtime, had a most happy effect indeed. So far as we can at present judge, it appears to have been really curative of the asthma. The last report says: “*Pari passu* with the asthma my cough and expectoration have gone, and I now have next to none of either.” In another case a highly informed and observant patient of Dr. Kingsford’s, who had found much benefit from the inhalation of iodide of ethyl for an asthma and bronchitis of twenty years’ standing, tried the citrate of caffein in two-grain doses every afternoon for a fortnight without any marked result. One day, however, being sadly worn out by a protracted attack of bronchial spasm which had lasted for eight hours, this patient took four grains of citrate of caffein in coffee, with the effect of obtaining immediate relief to the spasm, followed by three hours’ quiet sleep in his chair. The citrate of caffein appears to allay the abnormal excitability of the nerve-centres, and then repose ensues as a natural result. (Dr. J. C. Thorowgood, p. 150.)

Spasmodic Asthma.—*Arsenic Smoking.*—For asthma of the spasmodic kind, or having that tendency, smoking arsenic—a practice originally derived from China—is highly useful. But the regulation of the quantity used requires great care. Messrs. Savory and Moore made, at my suggestion, cigarettes of filter paper, burning almost without ash, each containing a definite quantity of arsenic, which answered the purpose well. (Dr. A. Leared, *British Medical Journal*, Dec. 28, p. 951.)

HOOPING COUGH.—*Atropia.*—I was led to try atropia by reading Trousseau’s remarks concerning the oftentimes beneficial results obtained from belladonna; for it appeared to me that if the experimental treatment was to be carefully pursued, more reliable preparations than the extract or the powder of belladonna ought to be used. And the alkaloid seemed to be more likely to prove useful—first, because of its unvarying strength; secondly, because the dose could be the more easily regulated in consequence; thirdly, because it is nearly tasteless. I therefore chose the solution of the sulphate of atropia (B.P.), which contains 1-120th of a grain in each minim—a most convenient strength; and in every case directed that it

should be administered in the morning fasting. I commenced, then, over four years ago to treat all cases of whooping-cough solely with the solution of sulphate of atropia, from infants two months old to the adult. It required some little time to find out the average dose to begin with; but I now begin with 1-120th of a grain (or one minim in a drachm of water), in children from one to four years of age, either diminishing or increasing the dose as occasion dictates; and, except in very severe cases, only order it to be given once a day; but when the nightly paroxysms are very severe, I order half the dose to be repeated about an hour before bedtime. The results that follow its administration may be summed up thus:—1st. There is a steady diminution in the *number* of paroxysms. 2nd. There is a diminution in the *duration* of the paroxysms. 3rd. There is a change in the character of the “hoop,” as if the vocal cords were not so closely approximated. Further, if the atropine is withheld, the beneficial effects derived from it subside. (Mr. A. Wiglesworth, p. 140.)

AFFECTIONS OF THE DIGESTIVE SYSTEM.

ANASARCA.—*Surgical Treatment of.*—The following method of treatment has been in use at Charing Cross Hospital for some months; it has been found to increase the patient's comfort considerably, while in no instance has it been followed by ulceration, sloughing, or cutaneous inflammation; it appears, indeed, to substitute a perfectly safe means of getting rid of the fluid for those which have hitherto been attended by much risk. The legs having been well oiled, and a macintosh sheet placed under them, about twenty or thirty punctures are rapidly made in their sides with a stout straight needle or hare-lip pin, care being taken that the needle is passed deeply into the subcutaneous cellular tissue. Some sponges which have been well wrung out in a solution of salicylic acid are now placed against the punctures, so as to absorb the dropsical fluid as it transudes; these sponges as they become saturated are squeezed out, and again passed through a solution of salicylic acid before being replaced against the patient's skin. In this manner renewals may be required every two or three hours, and several pints of fluid may be drained away during the first twenty-four hours, the whole process being possibly completed in four or five days, at the end of which time the punctures are usually healed. By the use of salicylic acid in the manner described, decomposition of the transuded fluid is obviated, the sponges are kept free from fœtor, the skin is not irritated, and cutaneous inflam--

mations of a low type, with their attendant evils, are entirely prevented. (Dr. H. A. Wickers, *Lancet*, Jan. 25, p. 118.)

CARDIAC DROPSY.—*Citrate of Caffein.*—Caffein, in the form of a soluble citrate, has been used in doses of a grain in tic-douloureux, hemicrania, and varied forms of neuralgia, but with doubtful success. We are indebted to Prof. Gubler for drawing attention to the special property of the drug in the form of citrate or bromhydrate in doses of from iv. to viij. grains, as inducing abundant and instantaneous diuresis in cases of cardiac dropsy, either when given hypodermically or by the mouth. Whatever may be the true theory of the action of citrate of caffein, the opinion is practically forced upon me through marked success in certain cases of the same type, and equally marked failure in other cases of a different type, that the caffein in doses of from gr. iij. to gr. vj., is a diuretic and cardiac stimulant of great value in cases of cardiac dropsy where a dilated, feeble, and irregularly contracting heart undergoing progressive mural decay is the main clinical and pathological element to be contended against. (Dr. L. Shapter, p. 157.)

EXPLORATION OF THE ABDOMEN IN OBSTRUCTION OF THE BOWELS.—Exploration of the abdominal cavity is, to my mind, justifiable on two cardinal grounds. Firstly, the mere opening of the peritoneal cavity is not necessarily a dangerous operation; secondly, there are many cases of obstruction of the bowels which must prove fatal unless relief can be given, which can only be rightly directed by means of exploration of the abdominal cavity. As to the impunity with which the peritoneal cavity may be opened, I need hardly remind you how constantly this is proved in operations for hernia. We think nothing of pulling out and handling coils of intestine; and we rarely look for danger unless the bowel have been damaged by too long continued strangulation. It is a common saying, that it is not the operation which proves fatal, but the delay in operating; in other words, the damage to the bowel by delay, not the damage to the peritoneum by the operation. (Mr. T. P. Teale, p. 167.)

GASTRALGIA.—*Arsenic.*—Dr. Leared, in a short paper on the Therapeutic Use of Arsenic, says: "The remarkable power which arsenic possesses of curing severe forms of gastralgia, constitutes one of its most valuable properties." (Dr. A. Leared, *British Medical Journal*, Dec. 28, p. 951.)

HERNIA-KNIFE.—Mr. Richardson thus describes his new hernia-knife: I had it constructed with the view to prevent the bowel overlapping the blade in herniotomy. It can be

directed to the seat of stricture or point to be incised with great safety, and once within, or under the constriction, it is unerring in its action. In this knife (see woodcut), the broad back, as now made, is secured to the blade by the anterior end of the latter being somewhat button-shaped, the button passing beneath a ledge in the back. The latter is secured to the blade, posteriorly, with a screw. This back projects 3-10ths of an inch beyond the anterior end of the blade; and, laterally, about 2-10ths of an inch at its broadest part. It tapers towards, and terminates near, the handle. The blade for 6-8ths of an inch anteriorly has a cutting edge that terminates a little behind its greatest convexity. From this point to the handle the edge does not cut. The most convex portion of the cutting edge is 2-8ths of an inch in depth. (Mr. B. Wills Richardson, p. 257.)

OPERATIONS ABOUT THE MOUTH.—*Mode of Administering Chloroform for.*—In cases of operation about the mouth, as removal of the tongue, removal of the jaw, and more particularly cleft palate, the difficulty of maintaining anæsthesia has been a source of much trouble both to the operator and to the administrator of the anæsthetic. I have lately made use of an apparatus for the administration of chloroform in such cases as those just referred to, which overcomes the difficulties. The apparatus employed is one which enables the vapour to be injected through a small tube into the respiratory passages either by way of the mouth or nostril. The chloroform vapour is obtained by means of Dr. Junker's very ingenious apparatus, which is usually employed for the administration of bichloride of methylene only, though it was intended by its inventor for the administration of chloroform also. For the face-piece, however, which has, so far as I am aware, alone been employed hitherto in connection with it, I have substituted a flexible metallic tube of a size which can be conveniently introduced within the nostril when required. A tube of this kind possesses great advantages, for instance, in such a case as removal of the superior maxilla; when all available space is occupied by the operator and his assistant, the tube can be introduced by the mouth, and interferes with no one. I produce anæsthesia at the commencement by sprinkling chloroform from a drop-bottle on to a piece of lint held before the face. When the patient is ready for the operation to be commenced, I throw aside the lint, insert the tube into the mouth or nostril, as most convenient, and so maintain a uniform state of anæsthesia. (Mr. J. Mills, p. 255.) [See page 364 for woodcut of Mr. Mills's Apparatus, which was received too late for insertion in its proper place.]

AFFECTIONS OF THE URINARY SYSTEM.

EXTERNAL USE OF DIGITALIS AS A DIURETIC.—It seems possible, that digitalis may act as a diuretic by absorption through the skin. Of course it could only be used in a case in which its internal administration was difficult or impossible. In a case of acute albuminaria with suppression of urine for thirty-six hours, and attended by semi-coma, I ordered half an ounce of the tincture on a large linseed-meal poultice to be applied to the abdomen. Next day I was agreeably surprised to find my patient vastly improved, quite conscious and cheerful. The oedema was very much diminished. Respiration was easy, and the pulse nearly natural. I was informed that, in one hour after the application, a copious flow of urine commenced and continued all night, and, what was very remarkable, the urine, which the day before contained a large quantity of albumen, was now quite free from it. Convalescence was rapid, and she is now quite well. (Dr. P. C. Russell, Lurgan, Brit. Med. Jour. Dec. 21, p. 919.)

LITHOTRITY.—It is very desirable to remove the entire stone at a single sitting if prudent to do so, as it leaves the bladder emptied of fragments, and therefore free from a fertile source of irritation. And such has for a long period been my invariable practice, whenever I have thought it possible to effect my object without injuring the bladder by too much or by too prolonged instrumentation. I have often been able to empty the bladder at one application of the lithotrite, aided by that useful instrument the aspirator of Clover, the patient being under the influence of ether. I have taken a hint from Bigelow's aspirator, and, slightly modifying Mr. Clover's original instrument, have, I think, rendered the latter more powerful and perfect, while I have avoided some material disadvantages attaching to the former. The new one, in fact, combines the best qualities of both. Thus I have greatly shortened the channel between the bladder and the aspirator, by getting rid altogether of the long arched tube which enters the top of the American instrument, and making the end of the evacuating catheter enter directly, without curve, at the bottom; so saving many inches of the route which has to be traversed by fragments. The new instrument is very easily filled with water, its action is extremely powerful, no air can possibly enter during the process of using it, and the amount of débris withdrawn is at once taken out of the current, and remains undisturbed and visible to the operator throughout the proceeding. I am quite satisfied with evacuating catheters No. 16 in size, English scale (No. 26 French); larger than those are mostly dangerous,

and wholly unnecessary. There should be several of them, with openings and curves of different kinds, one variety acting better for one patient and another for another. (Sir H. Thompson, p. 264.)

STONE IN THE BLADDER.—*Diagnosis*.—If you have a certain set of symptoms presented, and if they be aggravated by movement and relieved by rest, depend upon it that you have a stone to deal with. Let me take the case of a man suffering from frequent, difficult, painful, and bloody micturition, all of which symptoms may be caused by an enlarged prostate which has become inflamed. You want to clear up the case. All you have to do is to ask the man whether he is worse at night or in the day. If it be a prostatic case, the man will tell you that he is just as bad at night as in the day, or even worse; whereas, if it be a stone case, the patient will say to you that he is not so bad at night, for the simple reason that the absence of movement brings relief. Take another case. A man complains of frequent, difficult, painful, and muco-purulent micturition, all of which symptoms may be caused by a bad stricture. Ask him if he is better or worse when he moves about or rides in an omnibus. If he suffer from stone, there will be a great exaggeration in the symptoms; whereas, if it be stricture, there will not be. I have enumerated so many symptoms that they may possibly be difficult to recollect. If, however, you can remember the words *pain* and *blood*, and that both are *increased* by *exercise*, you will rarely ever overlook a case of stone; for, depend upon it, if a man come to you and complain that he suffers pain and passes blood when he passes urine, and says that he is much worse when he rides, you may at once be certain that you have a calculus to deal with. (Mr. W. F. Teevan, p. 273.)

The Introduction of the Sound (as distinguished from a Flexible Catheter) when it cannot be passed.—A celebrated surgeon, being unable, after several attempts, to pass a sound on a gentleman, the patient sought the advice of M. Mercier, who, being equally unsuccessful, put into execution a plan which he had previously devised for such an emergency. The vesical extremity of a large soft catheter having been cut off in such a way as to leave a rounded end, a slit, about one inch long, was made on the concavity of the tube, and a strong ferrule fixed on the other extremity. Some lard or wax having been put into the slit, so as to make the end smooth, the tube is introduced into the bladder. A long, slender sound, consisting of two portions respectively thirteen and twelve inches in length, and capable of being screwed

together, is now taken. Its beak should be placed at a more obtuse angle to the shaft than usual, in order to facilitate the passage of the instrument through the soft tube. A movable handle having been screwed on to the vesical half of the sound, the surgeon holds the ferrule of the soft tube in his left hand, and passes the sound through the tube till the bulbous extremity of the instrument has fairly emerged from the slit. The tube and the sound are now to be seen *in situ* as when in the bladder. The next step is to remove the soft tube so as to permit of the free manipulation of the sound and the transmission of sensations unimpaired by the coating of soft tube. This is effected by removing the handle and screwing the long stem into the vesical portion. The soft tube can now be retired, and the long stem removed. The handle having been screwed on to the sound again, the instrument is ready for use. (Mr. W. F. Teevan, p. 277.)

STRICTURE OF THE URETHRA.—*Catheterism on Physiological Principles.*—The unconditional use of a catheter would, in a certain state of the parts, certainly intensify the difficulty by calling into play a new source of resistance, in the form of normal muscular antagonism, to its passage—a force that is ever on the alert to oppose the enforced passage of a foreign body through the urethra into the bladder. This automatic force can be brought under complete control by an act of volition, and not only so, but be made to impart to the strictured canal the greatest amount of patency and passivity of which it is capable. The means to this end consist in making the patient bring the sphincters or detrusors of the bladder and urethra into a state of absolute rest by voluntarily, but gradually, calling into powerful action their antagonists, the expulsors or accelerators, and using the catheter whilst the force thus elicited is kept in a state of strain. This mode of palsyng the detrusors has another advantage which anæsthesia does not possess, since it assists the surgeon by employing the urine as a dilator, and thus reduces the resistance of the stricture slit. (Mr. John Gay, p. 283.)

External Urethrotomy.—Mr. John Coulson was, I believe, the first to devise the following operation, which is now generally practised. It has been improved upon by Dr. Gouley, of New York, and attention has been drawn to it by Mr. Teevan:—The patient having had the rectum cleared, and the perineum shaved, is to be put into the lithotomy position, and placed under chloroform. A straight staff, with a groove running to within half an inch of the end, and terminating in a button, is passed down to the stricture. The urethra is

opened upon this groove, *not upon the point of the staff*. The staff is then made to protrude, and the button hitched in the upper end of the wound, so as to enable the assistant to hold up the urethra by it. The edges of the opened canal are held apart by toothed forceps, and the operator proceeds to search in the cul-de-sac for the mouth of the stricture, by means of very fine whalebone probes. These should be olive-tipped, but there is difficulty in getting them properly made in England. Having found a passage, a fine silver tube is passed over the whalebone, and if the whalebone has been in the bladder urine will flow through the tube. The stricture is then divided upwards with a tenotome, directed by the slit in the tube, and then one half of an elastic catheter is passed into the bladder upon the tube, and the other half having been passed through the meatus, the two parts of the instrument are united by a screw-joint, and the patient is removed to bed. The advantages claimed for this operation are:—1. The greater chance of finding an entrance to the stricture with a fine olive-tipped bougie, applied close to the face of the stricture. 2. The demonstrating that the bougie is really in the bladder, by passing over it a fine silver tube, and drawing off the urine, and so saving the mortification of laying open a false passage. 3. The metal tube serves as a guide for the passage of a catheter over it. 4. Diminishing the risk by dividing the stricture subcutaneously, instead of enlarging the original wound. (Mr. G. B. Morgan, p. 278.)

AFFECTIONS OF THE BONES, JOINTS, ETC.

CLUB-FOOT.—*An Improved Method of Treating*.—Well-constructed orthopædic apparatus is of very great value, and can achieve signal triumphs in this disease. But it has its dark as well as its bright side. Its expense puts it beyond the reach of the poorer classes. Gradual rectification by plaster-of-Paris bandages—a method we owe to our German brethren—is capable of simplifying and cheapening, to an important extent, the cure of the disease, and putting a really reliable and satisfactory method within the reach of even the poorest. The best age to commence treatment is from six to nine months. The treatment by plaster bandaging is exercised on the principle of putting the foot into as good a position as it can be made to assume, fixing it by plaster in that position till it has become natural to it; then taking off the plaster, rectifying the position still more, and again fixing it in the improved position, and so on until the foot is straight. If the clubbed-foot of a child under chloroform be grasped by the two hands of the operator and

steadily and firmly unfolded, the projecting points which ought not to project (such as the calcaneocuboid joint) being firmly pushed inwards by the thumbs, and the toes and front part of the foot unfolded and coaxed outwards by the fingers, a certain improvement of the position is first obtained; then strong resistance is met with in the attempt at still further rectification, as the bones, ligaments, and fasciæ, all the firmer structures in fact, refuse further to give way. But if, disregarding their resistance, the force be continued, and even augmented, not by fits and starts, but steadily and inexorably, not with reckless force, but with a watchful yet determined persistence in the proper direction, the foot is brought into a position much more nearly normal than might have been considered possible; the bones, ligaments, and other resisting tissues, thanks to their yielding properties at so early an age, are moulded or forced into a comparatively normal situation without any signs of rending or fracture being observable, and their powers of resistance seemingly exhausted, remain in the new position with so little tendency to spring back, that the slightest support enables them to be kept there until the plaster bandages have been applied. Ten minutes' steady moulding in this manner are more than sufficient in every case to obtain in a child of nine or twelve months of age a most pronounced and decided amelioration of the pre-existing deformities. The foot must be unfolded before the heel is brought down. Division of anything save the tendo Achillis is rarely required. The fashion of promiscuous division of all the main tendons around the ankle cannot be too strongly condemned. It must be remembered that such tendons as possess synovial sheaths cannot be divided without the certainty, or next to certainty, that they will never reunite,—a fact often experimentally proved and never refuted, and that is yet not sufficiently appreciated or known. Although a foot may serve passably well for locomotion after division of a number of its tendons, that is no reason for running a muck against them, under the delusion that they are the offending agents in the disease. It is quite an erroneous and unjustifiable line of treatment to divide the peronei, tibialis anticus, the tibialis posticus, or the flexor longus digitorum, each of which possesses a synovial sheath around its tendon. The child has all clothing removed from its lower limbs, and is laid upon a small table of the ordinary height, so that the operator can finish his work without the fatigue of prolonged and considerable stooping. A pillow is placed under its head, and chloroform is administered. Children bear chloroform well, and its administration is most desirable, as it renders the limbs passive and quiet during the rectification and bandag-

ing. The chloroform need not be deeply given, but it must be administered sufficiently to prevent the child moving during the application of the bandages. The operator grasps, say, the right foot with both hands, back upwards, holding the ankle joint and heel in his left hand, and the front part of the foot in his right hand, with both thumbs pressing on the outer edge of the sole, opposite Chopart's articulation, just below the thin scar-like patch of skin usually present on the most prominent part of the dorsum of the foot. Maintaining steady pressure with his thumbs, he now strives by a deliberate, gradually and cautiously increased force, exerted by his hand, to abduct, evert, and pronate the foot, so as to bring it into as natural a position as possible, but without any attempt whatever to bring down the heel. At first the foot yields readily, but soon ceases to move as desired, and the operator becomes conscious that greater force will be needed to effect his object. Renewing his grasp, if this has been rendered insecure by the improved position of the foot and wrinkling of the loose skin on the outer part of the dorsum of the foot, he continues his task by applying, with the same steadiness and caution, an increased force, in exactly the same manner as before. The force is slowly increased, with every now and then, if there seems to be any danger of fracturing the bones, intermissions of half a minute or so, during which it is merely kept up, and not further increased, until the resistance of the foot being by degrees overcome, it unfolds from its perverse position, and has not the same strong tendency as formerly to spring back again. After as much advance has been made as seems prudent, the hands are removed, and the foot, if the manipulation has been well managed, lies passive and improved in posture, flaccid, and with considerably diminished tendency to return to its old vicious shape. There are no marks of its having suffered from the proceeding; there are no signs of fracture, contusion, or even abrasion, but the foot appears simply more normal in shape. When this has been achieved the operator next proceeds to fix it with plaster-of-Paris in its new posture. It is bandaged from the roots of the toes to the insertion of the ligamentum patellæ with a single thickness of flannel bandage, one and a half inches in breadth and four feet long. Once the varus position has disappeared the tendo Achillis must be cut. It is no use trying to avoid its division. The heel will not come down without it. (Dr. Alex. Ogston, p. 190.)

EXCISION OF THE ELBOW-JOINT.—In the case of the knee-joint, excision is not advisable, unless in some very special circumstances, after the age of forty. In the case of the

elbow-joint, the operation may be performed at any age. In the case of the knee-joint you require carefully to consider the question of interference with the epiphyses, especially the epiphysis at the lower end of the femur, because if it is involved in the disease, or requires to be interfered with during the operation, the result is not satisfactory as regards the after-growth of the bone. But in the elbow the interference with the epiphysis does not appear to be of so much consequence. In the case of the knee-joint, if there is any question between excision or amputation, you give amputation the preference; whereas, in the case of the elbow, amputation is rarely resorted to, and not unless in very aggravated circumstances, when the disease is very extensive or the condition of the patient is very bad. The last difference to which I would refer is that, in the case of the knee-joint, the result desired is a stiff and strong limb, which will take its part in supporting the body during progression. In the case of the elbow-joint, on the other hand, we desire to have a new and movable joint, one which will allow the movements of the limb to take place in as natural a manner as possible. So that, in the case of the knee-joint, the rule is to take away as little bone as possible, while, in the case of the elbow-joint, you must take away sufficient bone to give the limb mobility. In cases of excision of the elbow-joint, the best incision is one made along the posterior aspect of the joint, a little to the inner side of the middle line. In the adult an incision of about five inches long is usually sufficient, but you never can do any harm by making it longer in the superficial parts, because it is of great consequence to have the deeper parts thoroughly exposed. In cases where there is a wound or sinus, or sore, it is convenient to include that in the incision. For instance, if on the lateral aspect you have such a wound or sinus, you may sometimes modify your incision by taking a curve so as to include the sinus or wound. If this be upon the anterior aspect of the joint, you must not of course include it. The great advantage of the straight incision is that it allows you to split the attachments of the muscles, more especially that of the triceps. If you divide the muscles transversely, the result afterwards is not so satisfactory as regards the strength of the arm. The principal textures to be avoided in excising the elbow are—the ulnar nerve, as it lies over the inner condyle, and the brachial vessels in front. The best way to avoid these structures is to make your incision in the first instance quite down to the bone, and then in your dissection to keep the point and edge of your knife close to the bone, taking care to divide all the muscles where inserted in the longitudinal

direction, and as close to the bone as possible. In fact, it is a good rule to separate and cut away a piece of the periosteum along with the muscle. The structure which is most likely to be injured is, as you know, the ulnar nerve. Of course, in ordinary cases, if you can see it, and draw it aside, you prevent any risk of injury. But sometimes, especially in cases of extensive synovial disease, you will find it impossible to see it, and therefore you must keep your knife close to the bone, and turn the parts aside, and if you do this carefully there is very little risk. But supposing it to be injured by accident during the operation, or destroyed in the progress of disease, you should search for the divided ends, and stitch them together with catgut. The fact that the nerve is injured, or has been divided in the operation, does not necessarily interfere with the usefulness of the arm, and is not sufficient to make you perform amputation. Then, as to the periosteum. It has been proposed to perform the operation subperiosteally, as it is termed. In regard to this operation, I would say that it is quite right, in cases where you require to remove a very large amount of bone, to preserve as much as possible of the periosteum. But in an ordinary case, I would not advise you to retain the periosteum, except at the points where the muscles are cut away; because if you leave it there the growth of new bone is generally too excessive, and you do not obtain a thoroughly movable joint. (Prof. T. Annandale, p. 182.)

EXTENSION APPARATUS FOR OBLIQUE FRACTURE OF THE FEMUR AND OTHER INJURIES AND DISEASES.—There is the very greatest difficulty in maintaining extension of the limb in cases of oblique fracture of the shaft of the femur by any of the ordinary means in use. The perineal band, in the experience of every one, is constantly slackening and requiring readjusting or tightening. To obviate difficulties of this nature, Mr. Stokes has invented his instrument. It consists of two narrow parallel side splints: one for the inner and one for the outer side of the limb. These are made to slide one on the other, to enable the surgeon to adjust them so that the apparatus may be suitable either to a child or to an adult. These splints can be fixed by means of square-headed screws, which are governed by a key similar to what tuners employ in tightening the strings of a pianoforte. In the first appliance I made, these splints were made up of several pieces, varying in length and united by brass plates and winged screw-nuts. At the upper end of the inner splint, fixed on a hinge-joint, is a pad, which should be so arranged as to press on the perinæum. The pad being fixed on the hinge-joint facilitates the patient changing from the recum-

bent to the sitting posture, or *vice versâ*. The point of pressure remains the same, whichever position he may for the time prefer. I need hardly dwell on the great and obvious advantages to be derived from the patient being able to change his posture occasionally. The distal ends of the two side splints are attached to what may be called the extending portion of the appliance. It consists of a skeleton box, which is made to slide on the outer parallel splints; and these latter and the skeleton box are governed by the horizontal screw placed at one side of the box. The object of placing the screw at the side is to obviate the liability of the heel of the patient pressing on it, which might occur if the screw were placed in the centre. The distance between the two portions of the splint can be increased or diminished by rotating the screw from right to left, or *vice versâ*. To regulate the elevation of the limb, I have affixed a vertical screw. Across the skeleton box are stretched elastic bands, and on these the foot and lower part of the leg rest. A pad and piece of scored splintwood should be placed on the anterior aspect of the thigh, and kept in its place by a strap or belt surrounding the splint and thigh. The two free ends of a long and broad strip of plaister are then affixed to each side of the injured limb reaching nearly as high up as the fracture, an india-rubber ring having previously been placed in the loop below the heel. The ring is placed over the hook fixed in the horizontal bar at the lower end of the skeleton box. By rotating the horizontal screw; an amount of extending force can be obtained, which, in the great majority of cases of oblique fracture, will enable the surgeon effectually to restore the normal length of the limb. (Mr. W. Stokes, p. 200.)

MORBUS COXARIUS.—This is a disease which in a large number of cases has a purely constitutional or strumous origin, and may occur without any injury, or from so slight a shock to the joint that we cannot, strictly speaking, assign the term traumatic to it. Aspiration, to be successful, must be performed in the earlier stage of effusion or of pus formation, and hence the importance of detecting the first sense of fluctuation and watching closely the symptoms of approaching abscess in or about the joint. Though we may be guided by the situation of the sinuses, yet it is extremely difficult by means of the probe to ascertain the extent of the joint implication; and in those cases where sinuses form and suppuration has occurred without accompanying symptoms of waxy degeneration, the propriety of excising the head of the bone should be considered, and the earlier this step is taken, when once the necessity for it is foreshadowed by the urgency of the case, the better. (Dr. H. Macnaughton Jones, p. 215.)

RESECTION OF THE TARSAL BONES FOR NEGLECTED TALIPES OF ADULTS.—From the results which have ensued in several reported cases, surgeons will be justified in looking upon any patient with club-foot, whatever his age or however great the severity of the deformity, as remediable, if not perfectly curable, by resection of one or more of the bones of the tarsus. The operation may be rendered bloodless, or comparatively so, by the use of Esmarch's bandage, and the antiseptic method of treating such operative cases deprives them of the danger which would otherwise accrue from opening so many joints and synovial sheaths without such precautionary measures. Tenotomy will remain, as before, the operation for the relief of club-foot in children and infants; but in adults, and in many of the severe forms of talipes, where tenotomy has been resorted to and has failed, resection of one or other of the tarsal bones will be found a useful addition to our catalogue of surgical operations. (Mr. J. F. West, p. 220.)

SPINAL DISEASE.—*Sayre's Jacket.*—Experience of Sayre's treatment establishes the following results:—Pain is arrested immediately. So far as his spine is concerned the patient is at once able to sit upright and to walk about. Control of the lower extremities, when diminished by debility or paralysis, is rapidly restored in many cases. Abscesses close. The spinal column loses much of its abnormal curve, and consolidates in the improved position. In lateral curvatures a permanent increase of stature is often obtained. Finally, cure is more rapid, and ensues in a manner far less irksome to the patient than by any other method yet devised. Nevertheless, it must not be supposed, as patients are too apt to do, that the plaster shell will enable the patient at once to behave as if he were already cured, and to indulge in any exertion or exercise that may please him. Indeed, Sayre's treatment has this disadvantage: it gives the patient such complete support that he rejoices in his newly-gained power of locomotion beyond the limits of prudence. (Mr. Berkeley Hill, p. 170.)

During repeated applications of the plaster-jackets several small modifications have been tried, and some found to be beneficial. 1. The chin-piece is a frequent trouble. We have found it useful to cut the part which receives the chin more deeply than were the original patterns. Also to line the leather with thick soft felt. The suspending straps should be attached just in front of the zygoma, and just behind the ear, and hitched on to the ends of the stretcher, not to the notches half way along. Thus pressure on the chin, jaws, and nucha is reduced to a moderate amount. 2. The best axillary slings

have a hard firm pad, resembling the cross of a crutch, such bear least disagreeably on the vessels and nerves. 3. The cost of the self-suspending apparatus is materially lessened by re-placing the tripod-stand with a pulley screwed into the ceiling, through which a rope is rove; the chin-strap and stretcher are all that are then required. 4. In adjusting the skin-fitting vest it is well to lace its upper edges together with a tape carried backwards and forwards over the shoulders before the lower border is fastened below the hips and perinæum. 5. Fitting the pads requires great care—every bony prominence must be protected by making a soft one of cotton wool project beyond it on either side, not *over it*. In lateral curvature the hips, especially that of the side opposite to the curve, are apt to gall. This is best prevented by a Welsh flannel roller carried round them several times outside the vest, or by a layer of thick felt. Cotton wool is apt to get lumpy in time and lose its effect. 6. It is most important to carry the lower edge of the shell nearly as low down as the pubes in front and the sacrum behind, just escaping the great trochanters, arching it a little at the groins lest it incommode the patient when he sits upright. (Mr. Berkeley Hill, p. 173.)

Mr. Power, Medical Officer, Her Majesty's Prison, Dartmoor, has found two inconveniences in Sayre's plaster jackets, viz., the friability of the plaster when dry, and the tendency of the jacket to become slack. The addition of gum-arabic, as used by Dr. Walker, obviates to a certain extent the former objection, but he does not think so entirely as solution of gelatine. The tendency to slackness is owing to the fact that all woven material shrinks when wetted, and, consequently, expands to its original dimensions when dry. A condition exactly the reverse obtains with paper. Therefore he has found coarse brown paper an excellent basis for the plaster. It has also the advantage that it can be applied in a single piece (with as many layers as needful) to the parts requiring support. The method he pursues is as follows—whether as a spinal support or a splint for fractured ribs or limbs. Coarse brown paper, shaped to the size and form required, is immersed for a few minutes in warm water. The superabundant moisture having been removed, the plaster, moistened with thin solution of gelatine, is spread over one side; over the plaster is laid another sheet of paper similarly moistened. The whole is now applied with suitable bandaging, and left to dry. Any number of layers can be subsequently superadded. When dry, the jacket or splint will be found to have contracted, not slackened, and also to be some-

what elastic. (Mr. R. E. Power, British Medical Journal, March 15, p. 387.)

Angular Curvature of Spine treated by a Plaster Jacket applied in the Recumbent Posture.—1. The main object of the treatment of angular curvature of the spine should be the maintenance of the affected bones and joints in a state of absolute rest, and that in the position most favourable for the cure of the disease without deformity. 2. This position is found when the patient is placed comfortably in a recumbent posture. 3. By the application of a plaster-of-Paris jacket, as recommended by Sayre, the bones may be fixed in this position, so as to retain it when the patient rises and moves about. 4. The only way in which such a jacket can be applied with the patient recumbent, is by the method which I have demonstrated. 5. This method depends for its practical facility on the application of the many-tailed bandage and the use of plaster-of-Paris mixed as I have directed. 6. The adoption of the recumbent posture dispenses with the inconvenience and serious risks of suspension, while all the advantages of Sayre's method are secured for the patient at a minimum of trouble to the surgeon. (Dr. T. J. Walker, p. 175.)

ULCERS AND OTHER DISEASES OF THE LEGS.—*Martin's India-rubber Bandages.*—In cases of enlarged bursæ, œdema and anasarca of the legs and arms, varicose disease of the legs, phlebitis, especially in varicose ulcers, try Dr. Martin's india-rubber bandages. Bandages of varying length, width, and thickness are employed. In a few cases it is desirable to apply a bandage from the foot to the groin. The length of such a bandage should be about seven yards; its width three or three inches and a half; its thickness, if the case be one of phlebitis or varicose disease, No. 21 of Stubbs' wire gauge; No. 19, if more support be needed, as in a case where, with other disease, there should be weakness or disease of the joints. If the case be one of eczema or other cutaneous affection, only a much thinner bandage will meet all required indications which, in such cases, are not pressure and mechanical support, but exclusion of air and profuse diaphoresis and its results. In disease or injury of the ankle, I use a bandage five feet long, three or three and a half inches wide, and No. 19 (Stubbs' gauge) in thickness. For the knee, one of seven feet and a half, and the same width and thickness as for the ankle. In many cases of knee-trouble, I prefer a bandage fifteen feet long, three inches and a half wide, and No. 19 thick, to be wound from the foot upward, including the knee; for, if applied to the knee alone and

with sufficient closeness, an undesirable amount of oedema of the leg would be induced. All the bandages have a strip of strong linen inserted in one end, to which is strongly sewed a stout double tape eighteen inches long. The material is what is here technically called "pure rubber," *i.e.*, the best Para india-rubber "cured" with the minimum of sulphur necessary to produce with heat that wonderful effect of vulcanisation which, if properly accomplished, renders the gum practically indestructible and of perfectly and permanently resilient elasticity. The bandages which formerly I was obliged to have manufactured for my own use are now produced by the manufacturers of india-rubber throughout this country, and can be obtained from all the dealers. If there should arise any demand for them in England, they can, of course, be produced there as well or better, and certainly at a lower price, than in America. For the treatment of ulcers of the leg, its most important and frequent application, the bandage should be three yards and a half (ten feet and a half), not *twelve* yards, as Mr. Hutchinson is made to say in the report, three inches wide, and of the thickness of No. 21 (Stubbs' gauge). It should be rolled up with the tapes inside. It is to be applied directly to the skin and surface of the ulceration without any lotion, salve, or other dressing whatsoever. The statement attributed to Mr. Hutchinson, that a piece of lint may be placed over the ulcer, has no authority from me. An essential point in the method is that *nothing whatever* shall be applied between the bandage and the ulcer. In a very few cases, a hyperæsthetic patient has been, or thought he (or she) was unable at first or at all to endure the rubber next the skin, and, in these cases (not one in a hundred), I have directed that a linen, cotton, or thin flannel "roller" should be applied to prevent the immediate contact of the rubber. I have done this rather than renounce the great advantages of the india-rubber bandage entirely; but it always delays the cure of the ulcer. In cases of uncomplicated varicose disease, or in many joint cases in which the important use of my bandage is the support and security it affords, there is no objection to the application of an ordinary bandage under the rubber; but, in cases of ulcer, eczema, and other cutaneous diseases, and many where the joints are affected, it is necessary, for the full attainment of the benefits of the treatment, that the bandage should be in immediate and close contact with the skin. The bandage should be applied in the morning, before the patient has for any length of time been on the feet. The best way is to apply it in bed before rising. One turn is to be taken just above the malleoli; then ob-

liquely round the foot and again up and round the ankle, just below and overlapping the first turn; and so spirally up the leg, round and round, without reduplication, to the patella, just below which any superfluous bandage may be wound, the tapes separated, carried in opposite directions, and tied. Each turn of the bandage should overlap the preceding from one half to three-quarters of an inch. If it be applied in bed, it should be wound with just enough snugness not to slip down. When the patient assumes the erect position, the leg at once enlarges, from the accumulation of blood in its veins, and the bandage becomes of precisely the requisite closeness of application, and may be worn all day without readjustment, no matter how active the exercise or labour of the wearer. If the bandage be applied *after* the patient has been on his feet, it should be wound with sufficient closeness to put the india-rubber at each turn *slightly* on the stretch. If it be properly applied, there should be no unpleasant sense of tightness, but merely of comfortable and easy support. The patient should wear the bandage all day; at bedtime, it should be removed. The limb as well as the bandage will be found bathed in moisture. The leg should be wiped dry; a piece or pieces of oiled linen rag should be applied to the ulcerated surface, and retained by a few turns of an ordinary roller. The bandage should be rinsed with clean water and hung up to dry, to be ready for reapplication the next morning. In some cases, where the secretions are foetid or the patient filthy, it is well to rinse the bandage in carbolised water. In such cases, I advise that the leg should be washed with tar or carbolised soap and water. This antiseptic measure is proper and advisable, but does not seem to be essential to the treatment; for I have seen many cases where there was an utter neglect of everything like cleanliness, and in these cases results were as rapidly and completely successful as in those in which the greatest regard had been paid to the proprieties. In the morning, when the bandage is to be reapplied, all oily or greasy matter should be wiped from the stem with a dry rag. This is the entire treatment: india-rubber bandages worn while in the upright position; a simply defensive dressing while lying down. In a few days after the bandage is first applied, papules, often in considerable number, appear: these, if the treatment be continued, very rapidly suppurate, discharge, and disappear, not to return. Each of these pimples indicates an obstructed follicle. They sometimes look quite angry, and even resemble minute furuncles. Under the bandage, they run a very rapid course, leaving the skin in a perfectly healthy and unobstructed state. (Dr. H. A. Martin, p. 233, 240.)

WOUNDS AND ULCERS.—*Paper Lint*.—These may be treated by common lint or paper lint, impregnated with antiseptics. Common lint which has been soaked in a boiling saturated solution of boracic acid is hung up to dry; after drying, the water passing off, the lint is loaded with boracic crystals. The lint so prepared is antiseptic, and is the form of antiseptic recommended by Mr. Lister in the treatment of ulcers. If boracic paper lint can be easily prepared, and if it is equally efficacious, a great saving will be effected. The boracic paper lint is prepared in the laboratory of the Edinburgh Infirmary in the same way as the boracic common lint. Paper lint can be used for almost every purpose that common lint is used. I have made use of it principally in the treatment of ulcers, as recommended by Mr. Lister. The ulcer is thoroughly rubbed with chloride of zinc solution (forty grs. to the ounce), the surrounding skin purified with one to twenty carbolic lotion, a piece of protective the size of the ulcer is then placed on the sore, and then a single or double layer (depending on the amount of the discharge) of boracic paper lint is laid over the protective, overlapping it in every direction. The lint is soaked in the lotion before application. The discharge passes out below the protective into the paper lint. After the first dressing (if this is sufficient to purify the sore), boracic lotion is used as being sufficiently powerful and less irritating than carbolic lotion. In very putrid ulcers it may be necessary to apply the chloride of zinc lotion twice before purification is complete. The frequency of the after dressings will depend on the amount of the discharge. As far as I am able to judge, paper lint will supersede common lint, not in consequence of its superiority, but in consequence of its cheapness. We have now a cheap paper dressing which can be impregnated with a non-volatile antiseptic, like boracic acid, *without interfering with the porosity* of the dressing. We have yet to get a paper dressing which will retain a volatile antiseptic like carbolic acid (giving it off at the temperature of the body) and which will still be porous. (Mr. John Chiene, p. 224.)

Pressure in Wound Treatment.—In wound treatment pressure is a most powerful and beneficial agency. It favours union by maintaining accurate coaptation, and prevents extravasation of blood and its products. When such extravasation has occurred, pressure is the quickest, the least painful, and the most successful agency in promoting its absorption. A ready method for immobilising joints is to break off strips of thin rough unglazed millboard, measuring about twelve inches in length and two in breadth. Placing them on a dish or metal tray, they very rapidly become quite soft on a jug

of hot water being poured over them. The uneven broken edges bevel nicely under pressure, and the narrow strips fit easily. I first place one strip vertically on each side of the joint, previously covered with a layer of cotton-wool; bandage lightly; then diagonal strips, and gently compressing bandage, other strips, and alternating layers of roller, may be added in proportion to the strength required. This lattice-work or diagonal bracing makes a perfectly-fitting and efficiently supporting case, quickly constructed and very easily applied. (Mr. Sampson Gamgee, p. 243.)

AFFECTIONS OF THE SKIN.

BOILS.—*Arnica*.—Dr. Planat, of Nice, claims that arnica has the power of aborting an eruption of boils with extraordinary rapidity, except when due to diabetes. His method of employing it is very simple. In order to render its action on the small vessels more energetic, he applies it directly to the inflamed spot, in the form of an ointment, of which the formula is as follows:—Ext. of fresh arnica leaves, 3 iiss; honey, 3 vss. If the mixture is too fluid, he added powdered lycopodium, or some other inert powder, until it acquired the proper consistency. It is then spread pretty thickly on a bit of oiled silk or diachylon plaster, and applied to the boil. It is rarely necessary to renew the dressing more than once in twenty-four hours. As a rule, two or three dressings are enough to make a furuncle abort. A curative action is also obtained by the internal administration of the drug. Dr. Palnat gives three to four drops of the tincture, largely diluted, every two hours; and he has seen the furuncular eruption disappear very rapidly under the treatment. (St. Louis Medical Journal, and Dublin Medical Journal, Feb., p. 112.)

CHILBLAINS. — *Gillibert's Liniment for*. — Venice turpentine twelve, castor oil six, and collodion thirty parts. This is to be applied by means of a pencil to the parts which are the seats of the chilblains, whether these are ulcerated or not. The application is to be renewed as often as necessary in order to protect the chilblains from contact with the air, until the cure is complete. (Union Méd., and Medical Times and Gazette, Dec. 28, p. 731.)

ECZEMA.—*Oleate of Zinc*.—To the several valuable preparations of the oleates for which we are indebted to Mr. Marshall, the oleate of zinc promises to be a most useful addition. I now wish to draw attention to its utility in eczema. Mr. Martindale, of New Cavendish Street, prepares it by stirring together one part by weight of oxide of zinc with eight fluid-

ounces of oleic acid as free from palmitic acid as possible, and, after letting it stand for two hours, heating it until the zinc is completely dissolved. On cooling, it forms a yellowish-white hard mass, which can be made into the consistence of ointment by the addition of one part of vaseline or olive-oil or two parts of lard or simple ointment. Vaseline is preferable, as it is not liable to change. The other preparations, soon becoming rancid, should be freshly made, and then answer equally well and are much more economical. Mr. Martindale has also spread it on thin paper, which is a convenient way of applying in some cases. The above preparations I have found very effectual in acute and chronic eczema in the discharging stage. In the dry stage, it is also useful; but in many cases more stimulant remedies cure more speedily. (Dr. H. Radcliffe Crocker, p. 229.)

AFFECTIONS OF EYE AND EAR.

EARACHE.—In cases of earache with acute inflammation of the membrana tympani, and especially if there is pain, bleed the patient at once, then use the remedies you otherwise would have done, and you will succeed with them. (Prof. T. Wharton Jones, p. 60.)

FOREIGN BODIES IN THE EAR.—The easy removal of foreign bodies from the ear depends upon two things that are indispensable. 1, A light small syringe, with a small nozzle, throwing a small jet of water suddenly and forcibly, must be used. 2, The auditory canal must be made straight, by traction outward of the lower lobe of the ear, at the moment of injecting water into it. (Dr. J. Marion Sims, p. 297.)

HYPOPYON KERATITIS.—*Section of Cornea in.*—Any foreign body impacted in the cornea or elsewhere should be looked for and removed without delay. The pupil should then be dilated, if possible; and for this purpose we use a strong preparation of atropine with glycerine (introduced into the eye with a camel's-hair brush frequently), and for which I subjoin the formula, as it will be found most useful in breaking up commencing adhesions:—Sulphate of atropia, thirty-two grains; powdered starch, twenty grains; glycerine, one ounce. (One drachm of the mixture contains four grains of atropia.) We should endeavour to allay the pain by the administration of morphia by the mouth or (better) hypodermically, and the application of hot chamomile and poppy-head fomentations. Should we succeed in dilating the pupil and relieving the pain, the frequent instillation of atropine drops (two or four grains to an ounce) and application of hot fomentations will suffice. It is important in all these cases

to give good food and a moderate amount of stimulants, preferably beer. Where atropine and fomentations fail, or where the case is already far advanced, bolder measures must be promptly resorted to. The best plan, according to our experience, is to make a *bold incision* through the lower third of the cornea, extending through its whole breadth, exactly similar to the incision generally made in the modified Graefian extraction of cataract. (Dr. A. Emrys-Jones, p. 295.)

IRITIS.—*Bloodletting*.—It may fairly be asked whether by abstention from bloodletting altogether, as is the present fashion, inflammations of important organs are not often allowed to run a prolonged and disastrous course, which might be prevented by a timely abstraction of blood in such quantity that the loss of it could be in no wise injurious to the patient? In cases of iritis when bleeding has been had recourse to at the early stage of the disease, the pain around or over the orbit, or in the temple, ceases, or is soon greatly mitigated, the feeling of depression of the strength—for it is only a feeling, not real weakness—is relieved, the vascular congestion of the eye subsides, the pupil yields more completely to atropia, while mercury operating with more effect, less of it requires to be given. Without bloodletting, convalescence will not, in similar cases, take place in double the time, or even longer, and that only imperfectly; for, as a result of such a prolongation of the inflammatory action, serious organic damage of the eye is liable to accrue in the shape of synechia posterior, and implication perhaps of the ciliary body, choroid, and retina; whilst by the prolonged treatment the strength of the patient is pulled down, and a great tendency to relapse from slight causes remains. (Prof. T. Wharton Jones, p. 56.)

MIDWIFERY, ETC.

ABORTION.—*Ergotin*.—I place great reliance upon hypodermic injections of ergotin in every case of uterine hemorrhage during abortion, even though only moderately grave. Small abscesses may sometimes ensue at the seat of puncture, but they are somewhat rare. The formula employed is as follows:—*Rx.* Ergotini gr. ii. 8 pts. vini rect. et glycerinæ aa f. 3 ss. M., of which gtt. v. equivalent to gr. $\frac{1}{6}$ of the ergotin are to be injected. (Dr. Noel Smith, New York Medical Record and Practitioner, Feb., p. 137.)

FORCEPS IN MIDWIFERY PRACTICE.—At the Rotunda Lying-in Hospital, Dublin, our established rule is, that so long as nature is able to effect its purpose without prejudice to the

constitution of the patient, danger to the soft parts, or the life of the child, we are in duty bound to allow the course of labour to proceed. But as soon as we find the natural efforts are beginning to fail, and after having tried the milder means for relaxing the parts, or stimulating the uterus to increased action, and the desired effects not being produced, we consider we are justified in adopting prompter measures, and by our timely assistance relieve the sufferer from her distress and danger, and her offspring from an imminent death. Why should we permit a fellow-creature to undergo hours of torture when we have the means of relieving her within our reach? The instrument we at first employed was the straight form, which at the time was generally used in Dublin, both short and long, according as the head was low down or high in the pelvis; but on very many occasions, particularly in the latter instances, finding them fail, as they slipped from off the head when it was above the brim, or it was at all impacted, we eventually had recourse to Barnes' double-curved forceps, which we have ever since found most satisfactory, being equally easy of introduction, and when applied grasping the foetal head so firmly that we hardly ever failed to deliver with them. (Dr. George Johnston, Dublin Journal of Medical Science, Jan., p. 43.)

The Long Forceps.—The head of the infant is, before birth, so wisely formed and adapted as to safely bear a considerable degree of pressure without injury; but although this is the case, yet it must be understood that there is a limit beyond which it cannot be endured without proving destructive to the life of the infant. I feel convinced that the pressure produced by the labour-pains can be longer and more safely endured than it can under artificial compression by the forceps. If, then, the pressure produced by the forceps be really so destructive to infantile life, it must be proper to adopt effective means to obviate this mischief. The handles of midwifery forceps in general are too long, and thereby afford the operator power to squeeze the head of the infant to such an extent as to be certainly destructive of its life. The handles of the forceps ought to be so short that undue pressure cannot possibly be made. In my opinion this instrument ought to be chiefly used as a tractor, and only limitedly as a compressing power. If the pelvic brim is so diminished in its measurements as to require the use of the long forceps, the sooner they are applied so much the better, before the pelvic structures have been subjected to such a degree of pressure as will assuredly produce a further diminution of space. In such cases, it is unwise to wait for the full dilatation of the os uteri before the forceps are applied after

the rupture of the membranes, as it is impossible for the head to be forced upon this aperture so as to dilate it. (Dr. T. Radford, p. 201.)

Tarnier's Forceps.—These forceps have received many modifications since Tarnier introduced them. What first suggested the idea of the instruments was the employment of the traction power in the ordinary instruments by means of tape passed through the fenestra, this being used as the tractor, while the handles served as compressors. By this means traction is brought to bear on the blades without the intervening powerful lever which exists between the blades and the handles when the latter are used as tractors. In this way, of course, the pull is direct upon the child's head, and the handles merely act as indicators of the course in which the child's head is travelling, while it has every freedom to follow whatever course is most easy for it to traverse, freedom to rotate being thus given to it. This, then, is the grand principle of Tarnier's forceps. The handles serve to place the blades in position, and afterwards act as indicators of the axis in which the head is moving, while all the force that is necessary to aid delivery is brought to bear on the child's head directly and solely. It is allowed, nay, encouraged, to take the easiest course it can pursue, and we can always tell in what direction to pull by the position which the handles occupy. Force is never wasted on the soft parts of the mother, and the fatigue endured when Tarnier's forceps are used is nothing compared to that one undergoes when the ordinary long forceps are employed. (Dr. R. Bell, p. 303.)

New Tractor for Obstetric Forceps.—I had often considered if some means could not be devised which in a difficult case would give the required power without the necessity of calling an assistant (even if one were always at hand). I was, therefore, much pleased by reading in a number of the British Medical Journal, for June 29th, 1878, a description of tractors invented by Mr. Morgan, of Lichfield, to be hooked into the fenestra of any long forceps *after introduction* (in imitation of a more portable and cheaper form of the principle of M. Tarnier's instrument), and which he says "applies the power in a new direction, viz., in the middle of the curve of the forceps instead of the long axis of the handles," which latter he connects together by an india-rubber ring, and while the tractors are being used are always at hand when required, and moving in the direction in which traction ought to be made form a safe guide in what direction to pull. When additional traction is required as in cases of convulsions, hemorrhage, &c., where time is very important, I slip

the rings of my tractors into an ordinary swivel, attached by metal band to a strap which I have previously buckled round my waist, and thus sitting quietly behind my patient can make any amount of traction *consistent with safety*, by simply leaning backwards, with the greatest ease to myself and without fatigue or perspiring profusely as I often did before and many do still. (Dr. Alex. Duke, p. 306.)

INDURATIONS OF THE BREAST BECOMING CANCEROUS.—Among the occasional difficulties of diagnosis of the scirrhus cancers of the breast, one of the most frequent is that in which a portion of the mammary gland appears to be merely hardened, as if by some slow inflammatory or cirrhotic change. The history of the cases rarely helps in diagnosis. Whether cancerous or not, the induration may have been found “by chance,” no pain or discomfort having preceded; it is often observed, scarcely changing, for many weeks or months. Signs more to be relied on are that the indurations least likely, or not likely, to become cancerous are indicated by degrees of hardness less than those of scirrhus cancers of the same size and observed duration. They are more tough than hard; commonly, also, less rounded, less nodular, and less well defined. I say, “they,” but I have never seen in a breast more than one induration at all resembling cancer. If there be many, they are not likely to be or to become cancerous. Moreover, mere indurations do not involve the skin, do not invade or infiltrate it, or produce in it any puckering or dimpling, as by drawing a part of it towards their own mass. In this, indeed, I think there may be an almost unfailing diagnostic sign. When the nipple or any part of the skin of a breast is drawn-in to a subjacent hardness, this is almost certainly cancerous. It may have been so always, or it may lately have become so; but there are few diagnostic signs more sure, even though the retraction or dimpling may not be visible unless in certain positions of the breast or certain methods of holding it. Such dimpling may, in very rare instances, appear when an induration is suppurating at its centre, but I do not know any other condition in which the retraction over cancers is imitated; and in this case the diagnosis may rest on the fact that the retraction over cancer takes place towards a part of the induration which is harder or not less hard than the rest, while that over suppuration is towards a part softer or much more elastic than the rest. Softening, or a feeling of elasticity, at or near the centre of an induration may always be regarded as a favourable sign, but the observer must take care lest he mistake for it the softness of a thick layer of subcutaneous fat over a deep-seated cancer. Such softness or elasticity probably indicates either

suppuration such as may happen in a chronic abscess, or else the gradual increase of fluid in a cyst formed by dilatation of a gland-duct within a toughened portion of the gland. This character should be carefully searched for. Few diagnoses in surgery are more difficult than that between some serous cysts and some cancers in the breast, and the sign mainly to be relied on is the presence or the absence of elastic tension on firm, deep pressure over the middle of the "lump." If now, with such signs as I have indicated, a diagnosis may be made that an indurated portion of a mammary gland is not cancerous, yet it must always be borne in mind that it may be becoming so. The plan which I have usually followed is to direct that the hardened and adjacent parts of the breast should be covered with belladonna plaster, and that the patient should take a drachm of liquor potassæ and two grains of iodide of potassium three times a day after meals, in not less than four ounces of any simple liquid. (Nothing destroys the nasty taste of the potash better than a little liquorice.) Of the curative influence of the liquor potassæ I think there is sufficient evidence. Hard mammary glandular tumours, such as I have referred to, should be removed without delay. (Sir James Paget, p. 335.)

INUNCTION WITH OIL IN SOME DISEASES OF CHILDREN.—

Inunction with oil, the application of olive or salad oil to the skin, is probably the most beneficial of all measures for the preservation of health and the strengthening of body in delicate children, not only to prevent an attack of croup, but in scrofula and consumption and other diseases more or less akin, and often complicated with the scrofulous diathesis, such as rickets, tabes, laryngismus, tinea, impetigo, &c., it is invaluable. In inanition from whatever cause, especially when produced by mal-assimilation or mal-nutrition, and combined with a dry and disordered state of skin, this practice of oil-inunction is of the utmost advantage. Sir J. Y. Simpson says,—“In the marasmus of children, I have more than once seen oil-inunction succeed and apparently save life when all other means and remedies had utterly failed. When the body is much reduced by morbid eliminations, or by acute or chronic disease,—as after the dysentery and diarrhœa of children,—oil-inunction sometimes forms the best restorative. In rheumatism and in the chorea of the young, when accompanied by debility, it is often serviceable.” The mode of using the oil will vary according to circumstances; but, generally speaking, a wineglassful, slightly warmed so that it may be more easily absorbed, should be applied every night at bedtime over the whole cutaneous surface of the trunk and extremities, especially

where it will be most readily absorbed, between the thighs, the flexures of the limbs, and the sides of the body. For obvious reasons the night-dress should be of flannel extending well up round the neck, and down beyond the feet, although children's night-dresses might always advantageously be of this material. To further promote absorption warm sponging of the body might at first be employed, but after the first two or three weeks this is hardly necessary, as less time and friction are required the longer the practice is kept up. The oil should always be rubbed in till the process of absorption is complete, requiring at first from fifteen to twenty minutes; afterwards from ten to fifteen minutes is all that is usually required. (Dr. J. Moir, p. 146.)

LABOUR.—*Injection of Warm Water into the Vagina in Certain Cases.*—Injections of warm water into the vagina in properly chosen cases accelerate the labour without causing any increase of suffering to the mother. The only instrument required, besides a bowl of warm water, is a Higgins' syringe fitted with a vaginal tube; but this apparatus can be improved by the addition of a yard of india-rubber tubing three-eighths of an inch in diameter joined to the vaginal tube so as to carry off the water direct from the vagina into a receptacle, thus avoiding wetting the bed. The water should be as warm as the patient can comfortably bear, and in practice it is advisable not to begin with water raised to the full temperature, but gradually to add boiling water until the temperature of about 105° F. has been attained. The injection requires to be continued from five to twenty minutes according to circumstances. But there is one thing which must be borne in mind, that, unless the injection be given with a due regard to temperature, it is totally useless, so that to avoid disappointment it is better to administer it oneself rather than to leave it to a nurse, unless she can be fully relied upon. The effect caused is the relaxation of the maternal soft parts, and sometimes in addition the labour pains seem to be increased. Besides this, the patients generally say that the injections make them feel more comfortable. (Dr. W. J. Kilner, p. 309.)

OVARIOTOMY.—Being a strong believer in the efficacy of the antiseptic treatment of wounds, which I first employed in ovariectomy in 1876, it has seemed to me desirable to administer internally such drugs as present at least the probability of checking, if not preventing, the septic process. It has been shown by experiment that the flesh of animals killed after a short course of sulpho-carbolate of sodium showed a marked tendency to resist putrefaction, and that

urine from a patient similarly treated resisted putrefaction. Further, the odour of carbolic acid has been detected in the breath of patients taking sodium sulphocarbolate. It seems certain, therefore, that this salt is quickly absorbed and carried through the system of living animals, the double salt being decomposed in the blood or tissues, and the carbolic acid eliminated by the lungs, and probably also in part by the kidneys. Thus, a large quantity of carbolic acid can be introduced into the system. Professor von Nussbaum's remarks as to the danger of gut-ligatures slipping from the sunken pedicle are no doubt true; but I think I meet the difficulty by tying the arteries in the pedicle separately, as well as the pedicle itself, in cases where the vessels in the pedicle are unusually large, or by mishap I have divided the pedicle too close to the ligature. I think that instead of, or in addition to ligatures (of gut) round the whole or portions of the pedicle, it would be well, in cases of broad pedicle, to employ a strong darning-stitch ligature, knotting the ends securely. Like Professor von Nussbaum, I have freely used carbolic acid solution within the abdomen. In one case, with bad adhesions in the pelvis, I *poured* warm five per cent. solution into the abdomen, afterwards sponging it carefully out. Nearly all ovariologists recommend the complete emptying of the sac before it is withdrawn from the abdomen. I think it will be found advantageous where there are adhesions only to draw off part of the contents, and then remove the trocar and tie the orifice, so as to prevent the escape of fluid. The sac is then to be by degrees withdrawn; and I feel convinced that there is much less difficulty in recognising the details of adhesions, especially if intestinal, when the sac is thus partially full of fluid than when it is completely empty and hanging in loose folds. When the sac-wall is very thin and weak, it will rarely be possible to empty it with a trocar; and I believe it will be found the best plan to incise it freely with a knife, and turn the patient on her side and let the contents run out. Much valuable time will thus be saved, and, in the end, with care, less fluid will get into the abdomen than in the ineffectual attempts to withdraw it by a trocar, while the cyst-wall is tearing continually, and each attempt to attach it to the trocar only leads to additional tearing. (Mr. T. W. Himee, p. 329.)

PAINFUL SITTING AND COCCYODYNIA.—A very easy experiment explains the theory of painful sitting. It is not generally recognised that the bowels are pressed upon by sitting; but it is a fact, as this case illustrates. When a woman sits upon

a seat, the pressure upon her hips, even although the deeper parts are protected by the tuberosities of the ischia, communicates pressure to the deepest parts in the pelvis; and, if those parts are tender, pain is the result. The experiment that I alluded to, as demonstrating what I have just said, is to place the hand upon the hypogastrium while the perineum is exposed. If you press with it in the direction of the axis of the brim, you push down the perineum and the hips. A very slight pressure upon the hypogastrium makes the perineum bulge, makes the hips descend. Of course, when the hips are pressed upwards, or the perineum is pressed upwards, you have an influence which is, in like manner, communicated back to the hypogastrium, and thus you have pain. Other cases of painful sitting are owing to what is known as coccygodynia. The commonest cases of neuralgia of the coccyx or of true coccygodynia, although they have tenderness, or rather sensitiveness, as a symptom, have no inflammation, no inflammatory tenderness. Now, this disease is common, and it is common in men as well as in women. This disease is generally easily cured, or rather it goes away, and the treatment of it is scarcely worth describing. It is the use of laxatives, hot bathing, sedative applications. In a severe and persistent case you might try the hypodermic injection of morphia, and it has been said to cure the disease. Whether it has done so or not I shall not answer for; perhaps time would just have cured it equally efficiently. (Dr. J. Matthews Duncan, p. 323.)

RELAXATION OF THE PELVIC SYMPHYSES. — These joints in the end of pregnancy become naturally loose and juicy, and a considerable increase of motion is permitted in them. The loosening of these joints becomes morbid very rarely. When morbid, it has been found sometimes, in a few recorded cases, to be so extreme as to produce hopeless lameness. The joints have been so relaxed, and present such an amount of mobility, that by no contrivance can they be fixed so as to enable the woman to stand. Cases of that kind are among the greatest of rarities, but cases of slight extraordinary loosening are not very rare. This disease is fortunately frequently spontaneously cured. It is natural to expect that, as in a cow the moving huckle-bones get fixed again after parturition, so in a woman the movable haunch-bones will get fixed after parturition; and the same may happen more slowly in the extraordinary or morbid cases I am describing. In such cases I have always encouraged a woman to walk, to brave out the pain if she could, because the irritation produced by the walking may conduce to the refixation of the joint. Cases of this kind do get better. The bones do get fixed again. Until

they get fixed there is one means which is of great value, that is, a very firm bandage around the pelvis. You give an artificial fixedness. (Dr. J. Matthews Duncan, p. 327.)

TENTING.—The operation of dilating the uterus with tents is one which is often difficult, and always more or less painful and dangerous. The object of this paper is to explain a few simple contrivances whereby the difficulties (and, therefore, to some extent, the pain and risk) may be diminished. The inside of the blades is furnished with small teeth, like those of a rasp. It closes with a simple catch, so that the hand is not fatigued, nor the attention distracted by the effort of holding it shut. The tent is held so firmly that it forms with the forceps practically one instrument, which bears a general resemblance to a uterine sound with Sims's handle. With this forceps a tent can be inserted the whole distance in any case where a sound can be passed. Of course it can be used either with solid or hollow tents. A great difficulty with laminaria tents is their tendency to slip out. They do not fall out, they are extruded. This is especially liable to happen when the uterus is flexed. The tent is seldom found quite free in the vagina, but with its point just below the point of flexion: with the cervix well dilated, but the os internum in *statu quo*. It is often recommended to retain the tent by plugging the vagina, but this is objectionable, as it increases the risk of septicæmia, by retaining the discharges. And it is often ineffectual, for if the uterus has much tendency to extrude the tent it will do so, maugre the plug. It is generally recommended to choose a tent half an inch longer than the uterus. But if a tent be taken about one-third inch less than the length of the cavity of the uterus, it can be passed quite into the uterus. A second longer tent should then be passed, if possible, the whole distance, otherwise just into the cervix, to dilate the os externum. It will not be extruded, for No. 1, resting with its base just within the os externum, is kept in place by it, and keeps the uterus straight and extended, so that it has no tendency to force out No. 2. In ordinary cases, if there are two tents in the os internum, and one in the os externum, the two ora will be about equally dilated when the tents are removed; because the os internum is much more contractile than the os externum. When a laminaria tent is removed it often looks as if a string had been tied round it at the os internum, while the os externum has left little impression upon it. There is, therefore, no risk of a tent being incarcerated by the os externum. Before attempting to introduce No. 2, No. 1 should be withdrawn by the string, till it

slightly projects from the os externum, otherwise the point of No. 2 will hitch against the base of No. 1. Provided care be taken to keep the point above the inner os, it can be easily replaced by the finger, as soon as No. 2 is *in situ*. During the whole time that the uterus is being dilated, the vagina should be syringed with warm antiseptic lotion about every three hours. This lessens the risk of septicæmia, eases pain, and facilitates dilatation. It is well also to thoroughly syringe the vagina each time that the tents are changed. This is done most safely with the help of a speculum. I think that the best instrument for introducing sponge tents is a simple stout spike with a shoulder to prevent the tent from being jammed upon it. Sponge tents are often very disappointing, dilating the cervix largely, and leaving the os internum in *statu quo*. The base swells first, and draws the point out of the inner os. This may be prevented by a disposition somewhat like that recommended for laminaria tents. (Dr. F. C. Coley, p. 115.)

MISCELLANEA.

ANÆSTHETICS.—Choose in every case the safest anæsthetic, having regard to the needs of the operation to be performed. First on our list as a safe anæsthetic stands nitrous oxide. Let that be given, then, whenever practicable. For the operations of dental surgeons, nitrous oxide is, as a rule, sufficient—as a rule, but not always, a more prolonged anæsthesia being at times required; and for this purpose ether should be given in preference to chloroform as the safer anæsthetic. (Dr. C. Meymott Tidy, p. 349.)

Dichloride of Ethidene as an Anæsthetic.—Dichloride of ethidene has been experimented upon by a Committee appointed by the British Medical Association, and it has been found to be an excellent and safe anæsthetic. After relating six cases in which it was so used, the Committee report:—Three features are of special interest. 1. There was no injurious effect observable on the respiratory mechanism, although in all cases the anæsthetic was given in such doses as to produce complete anæsthesia and muscular relaxation, and in one (No. 5) the patient was deeply under its influence for twenty-five minutes. The question, however, of the limit to which the agent may be pushed without injuring respiration is of such immense practical importance as to induce the Committee to make a further investigation on this point. 2. The pulse diminished in frequency and increased in volume, and in the deepest anæsthesia it was steady, regular, full, and compressible. There was no indication of failure of cardiac action

in any case : a result anticipated from what had been previously observed in animals. 3. There was never any pallor of the countenance or blueness of the lips, but, on the contrary, and even during the deepest anæsthesia, there was a healthy flush on the face and the lips were rosy-red. Taking into account the change in the character of the pulse and in the colour of the face, it would appear that, in anæsthesia from dichloride of ethidene, the blood still remains in a normal amount in the arterial and capillary systems, and does not tend to engorge the venous system and right side of the heart, as is apparently the physiological action of chloroform. The total result appears to the Committee to be, that dichloride of ethidene, as an anæsthetic, presents all the advantages of ether, without any of its disadvantages ; and that the following opinion of Steffen, given in Binz's Evidence of Therapeutics, p. 39, is correct in most particulars : "It is said to have the following advantages over chloroform, which it resembles in its ultimate action, namely, a pleasanter smell, the power of producing narcosis more rapidly, as well as without excitement or vomiting, more rapid recovery without after-effects, and altogether less danger." (Dr. Coats, Dr. Ramsay, Dr. McKendrick, pp. 345, 346.)

CODEIA AS A SEDATIVE.—In phthisis, it allays cough without disturbing the digestive system ; and, in the other class of cases, I have found it tolerated when opium and morphia were not. As an instance of the latter, I may quote the case of a medical friend, a member of a gouty family, a frequent sufferer from migraine and derangement of the liver, and well aware of his intolerance of preparations of opium. He complained of a troublesome cough, depending on slight catarrh of the trachea and bronchi, and, at my suggestion, tried codeia, with all the benefit and none of the ill effects of opium. I prescribe the drug in doses of a grain, dissolved in syrup of tolu. The French medical papers constantly contain advertisements of codeia syrup, and probably it is well known as a cough tincture in this country ; but I was not aware, and others may have been ignorant as I was, that it has those advantages over the preparations of opium and its other alkaloids. (Dr. R. Saundby, p. 353.)

PRACTICAL MEDICINE.

DISEASES AFFECTING THE SYSTEM GENERALLY.

ART. 1.—OBSERVATIONS ON THE PERIOD OF INCUBATION OF SCARLET FEVER, AND OF SOME OTHER DISEASES.

By Dr. CHARLES MURCHISON, Consulting Physician to the London Fever Hospital.

By 'period of incubation' I understand the time which elapses between the poison of an infectious disease entering the system, and the first manifestation of symptoms. This is the ordinary acceptation of the term, but I consider this definition necessary, because some authorities have calculated the period of incubation of the exanthemata from the time of exposure to the poison until the appearance of the cutaneous eruption, including therefore in the incubation-period that of the primary fever.

A knowledge of the incubation-period of the several infectious diseases is of great practical importance. Advantage is taken of this knowledge in the practice of vaccination as a preventive of small-pox. The development of the areola, which is necessary for the protective effect of vaccination, occupies about eight days; but, as the latent period of small-pox is usually about twelve days, it follows that, if a person exposed to the poison of small-pox be successfully vaccinated at once, he has time to escape altogether, or to have the disease in a modified form. And, in all of the acute contagious diseases, the knowledge in question determines the time during which it is necessary to keep in quarantine persons who have been exposed to the infection, and helps also in determining the mode of introduction of the poison, or the origin of an epidemic.

It is, however, not a little remarkable how vague and unsatisfactory our knowledge still is respecting the period of incubation of most of the infectious diseases. Dogmatic statements on the matter, the most widely different, are made by the authors of our standard works on medicine, founded too often upon deficient observation.

Small-pox.—In this disease the incubation-period is usually assumed to be twelve days when the poison enters the system in the ordinary way, and only seven days when the poison is introduced by inoculation.

Measles.—I am inclined to think that the incubation-period of measles, although subject to variations, is quite as definite

as that of small-pox. There is, it is true, some discrepancy in the statements of different authors respecting it; but much of this discrepancy, I believe, is due to a difference in the method of calculation. In different works we find the incubation-period set down as between ten and sixteen days, and as being most commonly fourteen days; but when we turn to the facts upon which these statements are founded, it appears that, in the case of measles, the incubation-period has, in the majority of instances, been calculated from the date of exposure to the poison to that of the first appearance of the eruption, and that, therefore, the stage of primary fever, which lasts three or four days, has been included in the period of incubation. Making allowance for this source of fallacy, the incubation-period of measles is usually about ten or eleven days. It is worth noting, however, that, just as in the case of small-pox, when the poison is introduced by inoculation, the incubation-period is shorter, or only seven days.

Hooping cough.—Little is known as to the duration of the latent period of hooping cough. According to Squire it is usually about a week; while, according to Bristowe, it is probably about a fortnight.

Typhus fever.—In this disease also there is no fixed period of incubation. In a paper published in the second volume of the 'St. Thomas's Hospital Reports,' in which I collected 31 cases where I had been able to determine the period of incubation of typhus fever, I arrived at the following conclusions:—

1. The period of incubation of typhus varies in duration in different cases.

2. In a large proportion of cases it is about twelve days.

3. In exceptional cases it is longer than twelve days; but it rarely, if ever, exceeds three weeks.

4. In many cases (one-third or more) it is less than twelve days, and occasionally there is scarcely any latent period, the symptoms commencing almost at the instant of exposure to the poison.

Enteric fever.—Several circumstances contribute to make it very difficult to obtain satisfactory evidence respecting the incubation-period of enteric fever:—1. The difficulty, in many cases, in deciding when an attack of enteric fever really commences. 2. The circumstance that in fever hospitals nurses and patients rarely take enteric fever. 3. The fact that, in private practice, when the disease has been imported into a healthy locality, according to my experience it very rarely spreads. 4. The difficulty there often is in determining, when a person is seized with enteric fever soon after changing his residence, whether he has brought the disease with him, or whether he has been merely predisposed to the disease by recent

arrival in an infected locality. From the facts collected in my former essay, already quoted, I came to the following conclusions respecting the period of incubation of enteric fever:—

1. The period of incubation of enteric fever is most commonly about two weeks.

2. Instances of a longer duration appear to be more common than in typhus or relapsing fever; but it is very doubtful if the incubation-period ever much exceeds three weeks.

3. The period of incubation is often less than two weeks, and it may not exceed one or two days.

Some observers, including the late Dr. Parkes, have thought that the period of incubation of enteric fever was shorter when the poison was imbibed with the ingesta than when it was inhaled; but, in many instances of very short incubation which are on record, the poison was apparently inhaled.

Scarlet fever.—The period of incubation of scarlet fever has been variously estimated by different authorities.

But hitherto the statements made upon the subject have been based, for the most part, on one or two isolated observations, and no attempt has been made to collect a large number of observations bearing upon the point. So much has this been the case that, in 1861, Trousseau declared that neither in measles nor in scarlet fever could the latent period 'be rigorously determined in the present state of our knowledge'; while, still later, Obermeier gave it as his opinion that the incubation-period of scarlet fever was unknown. In the *Lancet* for August 13, 1864, I published 13 cases illustrating the incubation-period of scarlet fever; and I came to the conclusion that while in rare instances there was no incubation-period at all, this, in most cases, varied from one to five days, and very rarely exceeded six days. I have now collected all the cases of scarlet fever, having any bearing on the period of incubation, that have come under my observation, or been communicated to me during the last twenty years. They are 75 in number.

[Dr. Murchison gives the details of these 75 cases, and then says:—]

Of the total 75 cases, in not one did the incubation-period exceed six days; in 73 cases it could not have exceeded five days; in 54 cases it could not have exceeded four days; in 20 cases it could not have exceeded three days; in 16 cases it could not have exceeded two days; and in 3 cases it could not have exceeded twenty-four hours. It also appears that the longest period of incubation made out in any of the cases was four-and-a-half days; and that in only two of the cases was it certainly as long as four days.

The above cases lend no support to the opinion expressed by

some writers to the effect that the latent period of scarlet fever is longer in adults than in children, nor to the statement of Thomas, in his recent article, that scarlet fever differs from all the other acute exanthemata in the great variation of its incubative stage. Also the common argument that scarlatina has not been transmitted by a nurse, or the medical attendant, because of the short interval that has elapsed between their first communication with the patient and the appearance of the disease, is hereby shown to be untenable.

[Dr. Murchison adds a series of observations recorded by other physicians as additional illustrations of the short period of incubation in scarlatina, amongst which]

Dr. B. W. Richardson relates that once when he was attacked, the effect of the poison was instantaneous. He had applied his ear to the chest of a patient suffering from scarlet fever, and was conscious of a peculiar odour emitted by the patient. He immediately felt nauseated and chilly, and from that moment he was able to date the commencement of his illness.

My observations and study of the incubation-period of scarlet fever have led me to the following conclusions:—

1. The duration of the incubation-stage may be only a few hours.
2. Probably in a large proportion of cases it does not exceed forty-eight hours.
3. It very rarely exceeds seven days.
4. Consequently, a person who has been exposed to scarlet fever, and does not sicken after a week's quarantine, may be pronounced safe.

Duration of Contagiousness in Infectious Diseases.—It is not my intention to discuss at length this question on the present occasion. With regard to measles, however, I may be permitted to observe that, while the cases in Group III. confirm the general impression that measles is very infectious in the catarrhal stage, before the appearance of the eruption, and thus explains the difficulty or impossibility of preventing its spread in schools, Group II. shows that, contrary to the opinion expressed by Panum, the poison continues to be given off during convalescence.

The cases given under the head of whooping-cough show that this disease also is infectious from its very commencement, long before the development of the hoop.

In my work on 'The Continued Fevers' I have discussed the question as to what stage of typhus is most contagious.

Many facts show that scarlatina has an infecting power from the earliest stage of the malady, although the comparative facility of preventing its spread in a school, when the first case

is promptly isolated, points to this power being much less during the first two or three days than in the case of measles. On the other hand scarlatina has the power of infecting over a period of many weeks. The ordinary practice of pronouncing a patient safe at the end of a month, or after the apparent cessation of desquamation is not always safe.—*Clinical Society's Transactions*, vol. xi., 1878, p. 238.

2.—ON SCARLATINA.

By CHARLES JEWEL EVANS, Esq., Medical Officer to the Victoria Dispensary, Northampton.

The following opinions are the result of the personal treatment, derived from notes taken at the time, of more than 400 cases of scarlatina which occurred in my practice in the town and neighbourhood of Northampton during a period of six consecutive years, viz., 1865 to 1870 inclusive.

In the first epidemic year, in almost every case from the onset the sesquicarbonate of ammonia was administered, but it was found that children did not take it readily on account of its pungency, which, however, was to a great extent concealed by giving it in milk. In a few cases where there was not much fever, chlorate of potash combined with the tincture of the perchloride of iron was given, quinine being occasionally substituted for the chlorate during the period of convalescence. Many children, however, could be induced to take no medicine at all, especially among the poor. During the same period I was in the habit of applying a solution of nitrate of silver, of the strength of twenty grains to an ounce of water, to the fauces and palate with a camel's-hair brush, or, what was better still, a piece of sponge attached to the end of a stick, in the severe anginous cases, and it generally had a very good effect.

In 1868 and 1869, in the majority of cases, the chlorate of potash and steel, as above mentioned, were administered, although there were but few anginous cases. In the second epidemic year the tincture of the perchloride was universally given from the first, generally in combination with the solution of acetate of ammonia; and in every case of the anginous form the fauces were painted or swabbed once or twice daily with the above-mentioned tincture in its pure state, and in several most severe cases I had the satisfaction of seeing the parts resume a natural and healthy appearance, though the process, I must confess, had to be long continued. Indeed, during the last year (1870), I have been so impressed with the value of the perchloride of iron, both given internally and applied locally, that I have relied almost solely upon it, and I should have no hesitation in doing so for the future. At the same time, I am bound

to confess, and it is a circumstance I cannot altogether explain, that the statistics given in the table do not tally with that impression; for I have already stated that the mortality in the anginous cases was 26 per cent. in 1865, and reached as high as 33 per cent. in 1870. If, as I believe, my figures are correct (and the notes of the cases and remarks on the result of treatment were all jotted down at the time), it shows how fallacious mere impressions may be, and that they require to be either confirmed or otherwise by subsequent experience.

In a few cases the sulphurous acid spray was tried, but with no marked beneficial effect; and the extreme difficulty of using such an apparatus in the case of children was found to be such an insuperable objection to its frequent employment that it was soon abandoned. The same remark would apply with less force to the case of adults, but even in them I found the simpler application the more efficacious.

In the British Medical Journal, some few years ago, a schedule was issued requesting the members to record their experience of scarlatina, according to one or other of the plans of treatment therein suggested, among those plans being that of cold affusion. I have never seen any tabulated reply to these inquiries, neither have I employed that particular method of treatment; but the mere mention of this and of some other plans suggests how difficult, nay, almost impossible, it is to carry them out effectually; and if not done thoroughly, no reliable inferences can be drawn from them among the lower classes of the community, with whom ignorance and prejudice so often combine with unsanitary conditions and insufficient appliances to thwart the best-intentioned efforts of the medical attendant. Inunction of the skin with melted suet or oil, the moment the cuticle begins to desquamate, as recommended by the late Dr. Budd, was universally adopted. It is a plan easily carried out under any circumstances, and should be thoroughly done every morning, and be followed by a warm bath every evening, until desquamation no longer takes place. It is also beneficial in the earlier stages of the disease, as it relieves considerably the pungent heat of the surface. As a rule, however, there is a difficulty in getting the process carried out for a sufficiently long period of time. On the setting in of dropsy, small doses of tartar emetic (one-twelfth to one-eighth of a grain), as recommended by Drs. West and Golding Bird, were found very useful; and the remedy, *par excellence*, towards the close of it, was the ammonio-citrate of iron, which soon restores a healthy tone and colour to the skin. Dry cupping to the loins when the urine was albuminous was of great service; and when there was much difficulty of breathing, I had the satisfaction of rescuing several patients from approaching apnoea

by the free and frequently repeated application of turpentine to the whole chest.

Where convulsions ensued, calomel and jalap purges, cupping or leeches to the temples, and the hot bath, sufficed in every case to restore the patient.

This paper was originally written (in a modified form) for, and read to, the members of the Northampton Medical Society, and it was accompanied by a carefully constructed map of the town and suburbs, showing the exact localities where every case of the disease existed, having regard to the drainage of the town, at the suggestion of the surveyor, to whom I am much indebted for important information on that subject.

As the map, however, possesses merely local interest, it is not reproduced here. For myself, I am unable to see any relation as cause and effect between the disease and bad drainage, except in so far as the latter, by polluting the atmosphere, and thereby deteriorating the health of individuals exposed to it, renders them less able to resist the influence of zymotic and other similar poisons. With enteric fever, on the other hand, we have strong evidence tending to prove that the disease is propagated by the drinking of water contaminated with sewage matter containing the elements of the disease, though every now and then cases occur which tend to support the theory of its contagiousness.

But in the case of scarlet fever, smallpox, measles, and the like, it appears to me that overcrowding is a far more powerful cause of the spreading of those diseases than any other; and that regular and periodical inspection of the houses of the poor with the object of checking the same is of as much importance as effectual drainage, valuable as that undoubtedly is.

From a careful study of the map and of the above statistics we have arrived at the following conclusions, viz.:—That scarlatina when prevalent is pretty equally distributed all over the town; that defective drainage has little or nothing to do with its spreading, for we find that it was equally prevalent in localities recently and deeply drained and in those where old and superficial drains existed; that the modern and improved drainage has not expelled it from places where it previously had its habitat; and in several localities where the drainage was known to be most defective, none or but few cases occurred. This, I am informed, is also in accordance with the opinion of Dr. Buchanan, who visited the town after the occurrence of the epidemic in 1870.

To prevent the disease spreading when once it has entered a house, complete isolation of the patient is absolutely necessary, and free and systematic use of disinfectants should be insisted upon. An excellent plan is to nail a sheet outside the room

door and keep it thoroughly wetted all over with carbolic acid. The simplest and most effectual means of disinfecting the furniture of the room, articles of clothing, &c., is by means of the fumes of sulphurous acid disengaged by burning sulphur. But in every large town it is very desirable that there should be a public disinfecting-room, to which all infected articles should be sent. This plan has, I believe, already been adopted in a few localities, a very high temperature being employed for the purpose.

At the latter end of 1876 scarlatina was again very prevalent, but owing to the period of the year, or other causes, it did not extend over so long a space of time as on previous occasions, nor was there so large a proportion of severe cases. Still it was sad to find that the bitter experience gained by the public in former years had been attended by no practical good. The same lamentable amount of ignorance or indifference on the subject, or both combined, was met with as before.

It is practically impossible in the houses of the poor to prevent the disease spreading, and the compulsory attendance of children at Board and other schools, except in the case of personal illness under a medical certificate, is, I believe, one means among many by which the extension of the disease is favoured. It is therefore only by its frequently severe nature and its extreme infectiousness being more thoroughly known and appreciated that we can ever hope to arouse the public to a more effectual adoption of preventive and remedial measures.

First and foremost must be mentioned the commonly-received fallacy that scarlatina and scarlet fever are not the same thing, it being almost universally believed among the lower classes that the former is merely a trivial and harmless variety of the latter. Once and for all, the public mind should be disabused of this deadly error, which leads to the fact of children being allowed to mix together in school or elsewhere without any precautions being taken.

Again, the fact that the disease sometimes appears in so mild a form as scarcely to merit the name of illness, or indeed to cause such, encourages the idea that it is a simple affair, not infectious, and needing no particular care or precaution; but it cannot be too widely known among the community that such a slight case as that supposed (and, fortunately, a very great number of the cases are of this kind) may convey infection in the most virulent form to another individual, who from constitutional or other defect may be less able to resist its inroad.

Further, slight cases of the disease, in consequence, perhaps, of being subjected to earlier exposure, frequently are followed by secondary symptoms of far more serious import than the primary affection.

I would venture to suggest that a few simple directions, such as the following (which might be amended or extended as thought desirable), should be printed and distributed among the houses and in localities where scarlet fever is prevalent, to arouse the attention of the public to the importance of the subject, much in the same way as was adopted by the late Dr. Budd in the case of typhoid fever :—

1. Scarlatina and scarlet fever are one and the same disease.
2. There may be only a red rash covering the body, ranging from a faint blush to a vivid scarlet; or a sore throat only, without any rash; or, thirdly, the two may be combined.
3. Generally, during convalescence, the skin peels off in small bran-like scales, or in larger flakes, especially on the hands and fingers, and on the neck.
4. A simple, mild case, with but slight illness, may convey the disease to another person in the most severe and dangerous form.
5. A person once suspected of having the disease should be immediately placed in the topmost room of the house, from which all carpets, articles of clothing, and unnecessary furniture should be removed.
6. The floor should be sprinkled frequently with solution of chloride of lime, solution of carbolic acid, or Condy's fluid; rags dipped in the same should be hung about the room; and, where possible, a piece of sheeting should be placed across the open doorway, and kept constantly wet with one of the above-named liquids.
7. All persons not required for attendance on the patient, especially children, should be strictly forbidden to enter the sick-room.
8. Immediately the skin begins to peel off, the body should be thoroughly rubbed all over with melted suet every morning, and be washed with soap in a warm bath every evening, the process to be repeated several times.
9. No children out of a house where scarlet fever either exists or is suspected to do so should be allowed to attend any school or mix with other children.

If patients suffering from scarlet fever could be removed to a hospital, as smallpox patients are, and all their articles of clothing, &c., be sent for disinfection to some public establishment, the mortality from this terrible disease would, I think, be greatly lessened.

When one considers that the cases above described all came under the care of one medical man, and that probably a far greater number would fall to the lot of at least two others, being, like himself, officers of a public dispensary, to say nothing of a further series of cases attended by the remaining medical men

in the town, one can form a tolerably good idea, not only of the extent of these epidemics, but of the severity of them also. Indeed, than an extreme example of the anginous form of scarlatina, I do not know a more distressing and formidable disease. — *St. Bartholomew's Hospital Reports*, vol. xiv., 1878, p. 53.

3.—SEPTICÆMIA AND SCARLATINA.

By N. FFOLLIOTT, Esq., Surgeon-Major, A.M.D., Peshawur.

The following case may prove interesting, when taken in connection with similar cases recorded by Mr. May and Dr. Braxton Hicks, in the numbers of Dec. 21st, 1878, and Jan. 4th, 1879, and may help in some measure to answer the question: "Does septicæmia occasionally simulate scarlatina?"

Private Chapman, of the Fusiliers, was sent with his company from Jumrood, to form part of the garrison at Ali Musjid, nearly a month after that fort was captured. He had been a few hours in the fort when, urged by curiosity, he strolled about the place, and, coming in the vicinity of the Afghan powder magazine, he threw a lighted match on the ground after lighting his pipe. This ignited a quantity of powder that lay scattered on the ground, which he had not observed; the result was that he was, as he put it, "blown up," being severely burned on the left hip and inside of the thigh, and on the arms and face. The accident occurred on December 21st, 1878, and the following day he was sent to Peshawur to the Base Hospital, for treatment. On the third day after his admission (December 25th), there was considerable constitutional disturbance; a bright scarlet rash was found on the abdomen, which spread over the whole body. The following day, the eruption was very vivid, resembling a boiled lobster. Several medical officers who saw the case pronounced it scarlatina. Precautionary measures were taken, and the case was isolated. The eruption continued for five days, and then declined gradually. After the eruption appeared, the febrile symptoms subsided, the temperature becoming normal. The highest temperature recorded was at the onset, when the thermometer registered 101 degrees. The tongue was slightly furred, never at any time becoming red, nor was there any soreness of throat or enlargement of tonsils. The cuticle desquamated over the whole body, as in true scarlatina, even the palms of the hands and soles of the feet not escaping.

Although scarlatina is not unknown in India, I may state that, after a service of twelve years in the majority of the military stations of the Punjab, I have not seen or heard of a single case of this disease among Europeans or natives. There was no such disease in camp, as far as can be ascertained. The

soldier was twenty-four years of age, and has been three years in India. He recovered from the burns in due course, without any further ill effects, and was discharged from hospital on February 3rd, 1879.

There can be very little doubt that the absorption of the septic poison into the circulation is capable of producing a scarlet rash on the body; whether this is capable of transmission by infection, I am unable to state. I doubt it very much, and think, when such is the case, the disease will be found to be true scarlatina.—*British Med. Journal*, April 5, 1879, p. 505.

4.—THE ETIOLOGY OF ENTERIC FEVER.

By Dr. ALEXANDER COLLIE, Medical Officer to the Homerton Fever Hospital.

The object of the present paper is to show that enteric fever is contagious, and by this we mean communicable by direct personal intercourse. It is of secondary importance, for the purpose of the paper, to determine what, if any, particular excretion may contain the fever-producing material; but we think it has been asserted without sufficient evidence that this "material" resides in the stools alone. On the assumption that enteric fever is a specific enteritis, and nothing more, there would be reason for this opinion. It is, however, something more. The severity of the general symptoms is not only not always, or even in the majority of fatal cases, in proportion to the severity of the internal lesions, but they are sometimes entirely *unlike* the symptoms produced by acute local inflammations. We not unfrequently see cases of enteric fever in which the symptoms during life point only to acute poisoning, and in which it would be impossible to say to what the acute poisoning was due: cases in which post-mortem examination shows lesions in no degree proportionate to the severity of the general symptoms. If, then, we regard the fever as the result of a poison affecting the body generally, and the small intestine incidentally, we see no reason why the poison should be limited to the intestinal excreta, and why that of the kidneys, the lungs, and the skin should be held innocuous. That there is no such limitation derives support from the fact that the anatomical changes occurring in the early period of the fever are not limited to the small intestine. The early enlargement of the liver and of the spleen indicates early changes in those organs; and the early bleedings of the nose, mouth, and bowel point to early blood-change. Add to all this that enteric fever, apart from diarrhoea and soiled linen, has its "stink," as typhus and small-pox: a stink which, to persons with good noses, is an aid to diagnosis in doubtful cases; and who will dare say that

this stinking matter may not produce the disease? As we have already said, it is of secondary importance for our purpose to establish what particular excretion produces enteric fever. This is a question to be determined by future observation, on the hypothesis that one or all of them may produce it. We believe that the cases which arose at Homerton, and the record of which forms the basis of this paper, arose from exposure to *fresh* enteric stools; but, whilst this is the proposition which will be maintained in reference to the origin of these cases, we do not mean to say that cases do not arise from decomposing or decomposed stools or from emanations from the lungs and skin.

Further, it will be our object to show that, whilst we do not deny that enteric poison is sometimes contained in filth and in drains, and that these may be now and then means of its conveyance, there is no necessary connection between filth-decomposition or choked drains and enteric fever; that, in short, the pythogenic theory is not supported by the evidence, and is practically dangerous to life, inasmuch as it diverts attention from the real sources of danger, viz., the sick person and the sick person's diseased excreta. Choked-up drains and decomposing sewage exist in abundance in many places in all parts of the country, and for long periods together, without giving rise to enteric fever. It would be easy to give numerous instances of this; but better could not be found than the outbreak of "fever" in the Westminster School and the Abbey cloisters in 1848, and that of the Peckham Police Station in 1857, referred to by Dr. Murchison (Continued Fevers, 2nd edition, pp. 473-4). In the former, "it was found that the disease (enteric) followed very exactly in its course the line of a foul and neglected private sewer or immense cesspool, in which fæcal matter had been accumulating for years, *without any exit*, into which the contents of several smaller cesspools had been pumped immediately before the outbreak of fever. This elongated cesspool communicated by direct openings with the drains of all the houses in which it (the fever) occurred." In the latter, it was found that an accumulation of sewage "had been going on for years" underneath the passage adjoining the room where the policemen sat, and where they had complained of "dreadful odours." It would, then, seem evident that sewage-decomposition *per se* cannot be the cause, or at least cannot be the efficient cause, of enteric fever, otherwise it would surely have shown itself earlier at Westminster and Peckham. Had the enteric fever appeared at a comparatively early period of these accumulations, and continued during these accumulations, now ceasing and now reappearing with change of residents—in other words, if the disease, like the sewage, had existed more or less continuously for years, and had disappeared with the disappear-

ance of the accumulated sewage—then, we think, there would have been reason to regard such accumulation and disappearance in the relation of cause and effect. An effect which appears suddenly after the existence “for years” of the particular condition of things which is supposed to be equal to its production, cannot be reasonably regarded as an effect of that particular condition; for the particular condition had existed “for years”; the people liable to suffer from its operation had existed, too; notwithstanding, there is no effect, as there should have been if the cause, *i.e.*, particular condition, had been equal to it—in the form of an outbreak of enteric fever. Accumulations of faecal matter and stoppage of drains did then exist “for years” with the necessary second factor—suitable subjects for attack; still there was no enteric fever. How, then, may we ask, can it be said that these two factors, accumulation of sewage and suitable subjects, are equal to the production of enteric fever when they exist “for years” without any such production? The best instance of the inefficacy of sewage-decomposition to produce enteric fever is that of the Thames in 1858-9, recorded by Dr. W. Budd. “For the first time in the history of man, the sewage of nearly three millions of people had been brought to seethe and ferment under a burning sun in one vast open *cloaca* lying in their midst. Stench so foul had never before polluted this lower air; river-steamers lost their accustomed traffic, and travellers pressed for time made a circuit of many miles rather than cross one of the city-bridges.” “With all this condition of the Thames,” says Dr. Letheby, “the health of the metropolis has been remarkably good. In the corresponding period of last year (1857), the cases of fever, diarrhoea, and dysentery attended in the city by the medical officers of the unions amounted to 293 of the former and 181 of the latter; but, during the past quarter (*i.e.*, the quarter of intolerable stench), they were only 202 of the former and 93 of the latter” (Typhoid Fever: Dr. W. Budd, pp. 148-9-50).

To facts of this kind we do not see what can be replied. Here at least is filth-decomposition on a scale never before reached, and yet the health of those subjected to its supposed enteric-producing effluvia is not only generally good, but enteric fever is actually less common than usual. Dr. Budd’s reasoning on these facts appears to us conclusive as against the filth-decomposition theory, and it is not open to a conceivable objection which might be taken to the conclusion arrived at from the Westminster and Peckham cases. In these, it might be urged that, previously to the outbreak, there were no susceptible subjects: an objection barely conceivable in regard of the thousands exposed on and around the Thames. If it were the rule to find enteric fever unusually prevalent where

filth was most abundant, there would be reason to regard them in the light of cause and effect. But the case is just the contrary. Enteric fever is as common amongst the rich and the well-to-do as among the poor and the destitute; in the better constructed houses of the former as in the squalid homes of the latter. If it be true that enteric be a fever begotten of ordinary sewage-decomposition, how does it come to be relatively as common in the comfortable homes of the rich as in the narrow dwellings of the poor? It might, we think, be concluded, with more show of reason, that typhus originates in some kind of sewage or filth-decomposition; for it is mostly confined to localities abounding in some kind of filth. As regards enteric fever and probably other contagious diseases, to wit, diphtheria, we do not think that the filth and drain theory will stand. This at least we will say: that, after diligent search, we have been unable to find evidence on which that theory can be supported. With Professor Trousseau, we would say: "*Ce serait nous engager sur le terrain des banalités que de vous parler de l'influence d'un air vicié par des émanations putrides, de l'usage des aliments gâtés, boissons corrompues, etc., toutes causes hypothétiques que rien ne prouve.*" The fever is, in our opinion, contagious, sometimes severely so, and then affects, not only whole families, but even whole communities. Having regard to this part of the subject, we would say, with Dr. Budd, that contagion is the "master fact" in the history of enteric fever.

The importance of the subject will not be denied; for if, as we say, enteric fever be, like scarlatina, small-pox, and typhus, contagious—it is of little importance how—the present mode of treatment must be altered, and isolation practised in enteric fever as in small-pox. We believe that our Homerton experience establishes this proposition; but, to show that this experience is not isolated, but part of a body of facts pointing to the same conclusion, before recording that experience, we shall give a short account of instances of contagion observed by eminent physicians in France and Germany.

"Opinions," says Trousseau, "are still divided on the question of the contagious nature of enteric fever; but the number of those who refuse to admit contagion diminishes daily" (*Clinique Médicale*, vol. i., p. 276, Paris, 1868). He then narrates several instances of contagion, of which the following are three. Enteric fever was imported into Carriol (Gironde) by a young workman who had arrived sick at his home. The family was composed of seven persons, living in a large well ventilated house. All were severely attacked, of whom three died. From thence the disease spread amongst those who had communication with the sick, and these removing, sometimes to very great

distances, carried the disease to a locality where it had never before existed.

On May 31st, 1857, a young man arrived at Audon-le-Romain (Moselle) from Paris, ill for some days of enteric fever. He spread the fever to the companions who visited him, and then to his father, brother, and two sisters. So long as field-work kept the inhabitants of Audon away from their houses, the fever, although it spread in the village, remained limited to a small number of inhabitants; but, when the harvest was finished, constant residence with the sick produced a general infection, and there were at one time forty cases in a population of four hundred and forty-two. "If," says Dr. Fourrier, Trousseau's informant, "sceptics see nothing here but a coincidence, where would they be able to see relations of cause and effect?"

A farm-lad aged 12, in the service of the Mayor of Bièvres, whose wife and daughter had had enteric fever successively, contracted the disease, and carried it to his village at Orgeval, a distance of about two miles, where no case of enteric fever existed. He gave the disease first to the relative who nursed him, and this relative gave it to another relative, who came from the opposite extremity of the village to help her. Thence the disease spread in the municipality (commune). But this is not all. A young domestic employed in a house at Orgeval, where there were cases, was attacked with the fever, and sent to her own home, a distance of three miles and three-quarters, where she carried the disease, which became epidemic. In concluding his chapter on Contagion, Trousseau admits that, in many instances, it is impossible to trace the source of the contagion (a remark, by the way, which is true of small-pox, than which there is perhaps no other admittedly contagious disease in which it is so often impossible to trace the source of the contagion); and that, whilst admitting contagion, it is not necessary to deny spontaneous origin. But he does not think that spontaneous cases arise from putrid emanations, "which prove nothing," but from age, overcrowding, and climate.

Liebermeister, who denies that enteric fever ever passes from person to person, admits that he has frequently seen it arise in hospital. "If," he says, "this occur during the prevalence of an epidemic, and the number of the cases be small, nothing worthy of remark has taken place. But, even if the number of the cases within the hospital should become exceptionally numerous, this is not to be regarded without further consideration as evidence in favour of the direct transmission of the disease. It is far more likely that the causes will be found in local conditions. If there be an infectious centre (Infections-herd) within the hospital, numerous cases of sickness may be

produced thereby; and that such an infectious centre (Infectionsherd) may readily form itself in a hospital containing numerous enteric cases (Typhuskranke) is, in the present state of our knowledge, easily understood. I have myself, since the year 1865, had frequent opportunity to observe such hospital infection. But my conviction that it does not pass from person to person has only been on that account strengthened." (Ziemssen's Handbuch, vol. ii., part 1, 2nd edition, p. 48, Leipzig, 1876.) We do not quite understand what Liebermeister means by infectious centre (Infectionsherd) and transmission from person to person; but, as far as we do understand him, his belief appears to us to be in opposition to his evidence. The plain fact remains that, in hospitals containing enteric cases, the disease does spread, and persons who have it not take it from those who have it; unless it be maintained that they take it from those who have it not. Liebermeister states that, in the six years he was connected with the hospital at Basle, from 1865 to 1871, there arose there forty-five cases; and that, in addition, "there appeared among the physicians, nurses, and servants so-called febrile and non-febrile abdominal catarrh, which must be referred partly to light infection with enteric poison (Abdominaltyphusgift)" [*ibid.*, p. 48]. He records, further, that, "in the years 1872-73, as I learn from a friendly communication of Professor Immerman, six to eight cases of enteric fever occurred within the hospital amongst physicians, nurses, servants, and patients; whilst almost all newly appointed assistants, nurses, and servants passed through so-called abdominal catarrh without fever" (*ibid.*, p. 49). And yet the disease does not, or rather Professor Liebermeister believes that it does not, pass from person to person. It does, however, as matter of fact, pass to persons; and, on Liebermeister's authority, we think we are justified in saying that enteric fever, when introduced into hospital, spreads as do typhus, small-pox, and scarlatina. The particular form of "set words" in which this important fact may be expressed is, we think, of minor importance.

It would be easy to multiply instances of a like kind. Piedvache's essay contains numerous such, and those of Dr. Budd are too well known to be more than mentioned. The valuable experience of the London Fever Hospital also supports our contention—*British Med. Journal*, Mar. 8, 1879, p. 341.

5.—TYPHOID EPIDEMIC ORIGINATING IN DISEASED MEAT. By Dr. WALDER, Assistant-Physician to the Zürich Hospital.

A most remarkable epidemic of typhoid fever, which seems clearly traceable to eating the flesh of a calf that had probably died of typhoid fever, is described by Dr. Walder, Assistant-Physician to the Zürich Hospital, in the *Berliner Klin. Wochen-*

schrift, No. 39, 1878. On May 30, 1878, a choral festival was held at Kloten, Canton Zürich, and was attended by about 700 members of neighbouring choirs. A public breakfast, consisting of stewed veal and sausages, took place at 9 a.m. At 3 p.m. there was a somewhat similar repast, with the addition of soup, potatoes, salad, and wine. Water was drunk by only a very few persons, and always mixed with wine. Of those who took part in these meals, about 500 sooner or later fell ill. The greater number were not attacked for two or three days after the festival, though a few were unwell on the next day. Dr. Walder calculates that 39 to 40 per cent. of all the cases were taken ill on the fifth and sixth days, and 90 per cent. within the first eight days. The general characters of the symptoms were those of typhoid fever of various degrees of intensity, from mild abortive forms to those accompanied by severe delirium, intestinal hemorrhage, and high fever. The epidemic presented two deviations from the ordinary run of typhoid epidemics—that the fever rose very rapidly at first, so as often to reach its height on the second day; and that diarrhœa was less common than usual, and less obstinate. Temperatures of 40° C. were not uncommon, and those of 40.5° to 40.8° were several times observed. Most of the patients exhibited the typhoid roseolar eruption, and the spleen was enlarged in all those cases which were examined at the acme of the disease. Dr. Walder states that most of the patients whom he specially examined had swelling of the external lymphatic glands, especially the inguinal, and that the swelling disappeared when the fever left them. In the course of the epidemic, the usual complications of typhoid fever occurred. Four cases relapsed, one of which, a youth aged sixteen, eventually died from perforation of the bowel and general peritonitis. When Dr. Walder wrote, twenty-seven cases of secondary infection had taken place, the patients having either been engaged in nursing those primarily affected, or in washing their linen, removing their motions, &c. Some secondary cases induced by sleeping in the same bed with primary were more severe than the latter, but as a rule the secondary cases were milder than the primary. The post-mortem examination of several fatal cases, four of which Dr. Walder reports *in extenso*, confirms the diagnosis of typhoid fever so as to leave not the slightest doubt as to the nature of the disease.

We now come to the probable source of infection of those persons who took part in the festival at Kloten. The greater part of the meat eaten on this occasion was supplied by the village innkeeper, who was also the village butcher, and all of it (veal, pork, and beef) had been pronounced by a professional inspector perfectly healthy, with the exception of forty-three pounds of veal, which were sent from a butcher at Seebach,

two days before the festival, and had not been examined by the inspector. The calf from which it came belonged to a peasant. It was only a few days old, and was probably only killed because it was certain to die very speedily from illness. It would not suck, it lay on the straw, cried out when touched as if in pain, and at the last kept lowing loudly. The evidence that the flesh of this calf caused the epidemic is very strong. Not only the partakers in the feast who ate this particular veal stewed with the other healthy meat were attacked, but families which took no part in the feast, and in which the children had had meat and sausages given them by persons who could not get through what was served out to them, suffered. The lungs of the above unfortunate calf were sold to a lady at Seebach, and the brain to the clergyman of the parish. Three persons who dined off the lungs made into stew were taken ill exactly like the members of the choirs, and the clergyman's family were similarly affected. The bones were sold to a dealer at Seebach, and his dog, which ate part of them, was ill for about a week. Moreover, several persons who bought meat either on the day of the festival or on the previous day from the Kloten butcher, also suffered, as did a number who had bought sausages from the same establishment, and persons who had taken no part in the festival, but had dined at the inn kept by the butcher, were laid up. Dr. Walder supposes, with good reason, that healthy meat became infected either by direct contact with the diseased veal, or by having been cut by the same knife.

It should be mentioned that there had been no epidemic of typhoid fever either in Kloten or the neighbouring villages for many years; and though, as a fact, there was a single case of typhoid fever at Kloten at the time of the festival, the patient lived a long way both from the place where the feast was held and from the butcher's shop. The water used for cooking and drinking was supplied from a hill on the other side of the village, through iron pipes. The first person who suggested the origin of the epidemic in the typhoid fever of calves or oxen was Professor Huguenin of Zürich, but his suggestion was long received with considerable scepticism, as scarcely anything was known about the occurrence of the disease in those animals. The correctness of his hypothesis, however, is strongly corroborated by Dr. Walder's discovery of the infection of two calves belonging to one of his patients suffering at the time from typhoid fever. A post-mortem examination of one of these animals showed intense swelling of the Peyer's patches throughout the whole of the small intestine, but especially in the lower part, with swelling of retro-peritoneal and mesenteric glands. The spleen was enlarged. The heart, the lungs, and liver, as well as the joints,

were all healthy. There were several small hemorrhages into the left kidney. No similar cases have ever occurred, before or since, among this farmer's cattle, and it seems most probable that the farmer himself was the infecting agent, and that during one of the fits of severe diarrhoea from which he suffered in the first week of his illness he must have passed a motion in the neighbourhood of the stall, as during this time he was continually attending to the cattle himself. Another case, in which a calf was almost certainly infected by a human being, occurred later on. Here a bucket which had been used for washing the viscera of two patients who had died of typhoid fever, and on whom a post-mortem was made, was soon after filled with water for the calves to drink, and it seems probable that some blood which remained on the outside was licked up by the calf, or else was transferred to the stockings of the cow-boy, which the animal was in the habit of licking. After an incubative period of exactly ten days, the calf was taken ill, and it was killed four days later. The pathological changes were exactly the same as in the other case, and microscopically the calves' intestines in both cases were indistinguishable from a human intestine in the same stage of typhoid fever.—*Medical Times and Gazette*, Feb. 8, 1879, p. 149.

6.—DIPHTHERIA AND TYPHOID FEVER.

By Dr. CLEMENT DUKES, Physician to Rugby School.

That the same poison which originates typhoid fever often causes diphtheria; that the two diseases often occur side by side, in different individuals, in the same house; and that, therefore, we must look for their same source of origin, the following cases show.

1. On Oct. 30, 1875, I was called in consultation some distance in the country, to a large private school at V——, to see Mr. R., who was supposed to be suffering from typhoid fever, though he was not. There were, however, five or six cases of typhoid fever in other rooms that I saw. While these cases were in progress, and within a few days of my visit, a younger child developed pharyngeal diphtheria and died. These cases occurred in a large house, standing by itself in several acres of ground; they were traced to the well-water (there was no other water-supply); there had been a great rainfall, the cesspools had filled and overflowed, and, the well being near and on a lower level than the cesspools, the overflow had found its way into the well, which was marked by making the side of the well at which it entered slimy and blackish.

2. At the beginning of January, 1875, Mr. T., of Rugby, developed diphtheria, which was followed by paralysis. In the

same house, on January 15th, 1875, Mr. T.'s servant sickened with typhoid fever, went through a severe illness, but recovered. During the last days of December, 1874, there being something amiss with the pipes which carried the water-supply from the town to the cistern, the water was cut off *for one day* to remedy the defect. On that day a pump in the yard at the back of the house was used for their water-supply; this well was much fouled from adjoining stables. There was, however, another possible source of poison; for the trap to the water-closet was defective, and allowed an escape of sewer-gas into the house. Both sources of poison were remedied, all the family were sent out of the house, and we had no further cases.—*British Medical Journal*, Jan. 25, 1879, p. 113.

7.—TYPHOID FEVER DURING INFANCY.

By Dr. ALFRED WILTSHIRE, Joint-Lecturer on Midwifery and Diseases of Women and Children at St. Mary's Hospital.

[The following is a well marked example of a case of enteric fever in an infant aged six months.]

On Sunday, February 23rd, I went to Redhill to see (with Dr. Walters of Reigate) a little boy who was suffering severely from a relapse of typhoid fever. After our consultation, Dr. Walters was so kind as to invite me to see, in the adjoining house, an infant aged six months, who presented a typical example of the disease. The child manifested the ordinary symptoms, and the eruption was abundant and pathognomonic. I understood that the disease had been contracted through the food, which was partly, if not wholly, artificial.

Dr. Walters, whom I have to thank for kindly permitting me to make this reference to the case, says, in a letter with which he has favoured me: "I remember seeing a case in as young a child in a former epidemic here, which was due also to contaminated water-supply. It was also characterised by a very copious eruption." Both Dr. Walters's cases recovered.

Typhoid fever in *young* infants is rare. I have myself seen it in infants on several occasions, but I do not remember ever before having observed it at as early an age as in Dr. Walters's cases. Probably the exemption of young infants is mainly attributable to their mode of feeding; that is to say, children reared solely at the breast escape (unless their mothers suffer), while those who are either wholly or partly hand-fed are exposed to it. This at least accords with my own observation. It is well to bear in mind in this connection that, very commonly, young infants—*e. g.*, those under six months—escape the invasion of the acute specific diseases, even when other children of the family suffer. It is to be feared that typhoid

fever is not unfrequently overlooked in young children; "gastric fever," "infantile remittent fever," "low fever," and "slow fever" being assigned as the cause of illness.—*British Medical Journal*, March 22, 1879, p. 427.

8.—ENTERIC FEVER IN INFANCY.

By Dr. WILLIAM S. PAGET, Great Crosby.

Dr. West, in his work on Diseases of Children, considers typhoid fever exceedingly uncommon before the age of two; and, in the *Journal* of March 8th, Dr. Walker gives the notes of a case in a child aged fifteen months, when Dr. West was called in consultation. In the *Journal* of the same date, Dr. William Thomson discusses the contagious nature of the disease, and appears to think that one reason why adults come into contact with it, and yet do not contract it, is because they have already had it in infancy, and thus are shielded from a second attack; from this I infer he considers that the infantile attack in many cases probably passes undiagnosed. It would be interesting to know what percentage of cases occur during the first two years of life. I am at present in attendance upon a very typical case in a child eighteen months old, where all the characteristic symptoms are present. She was taken ill on March 3rd, and I saw her first on March 5th; for three days, the symptoms pointed to nothing very definite. The temperature was 99 deg., the pulse 120. The tongue was thickly coated. The child was restless, and disinclined for food. The bowels were rather costive, and the stools inclined to be clay-coloured. On March 8th, I detected some tenderness in the right iliac region, and two or three spots of suspicious character on the abdomen. On March 9th, the temperature was 101·6 deg., pulse 140. There were eight or nine spots on the abdomen; distinct splenic enlargement; and slight distension. Three semi-solid stools of ochrey character were passed on March 10th. There were numerous spots on the back. Temperature 102 deg. The motions were three in number, of the same character. There was a good deal of twitching about the hands and face. During the next five days, the symptoms continued much the same; temperature 102 deg. on an average (1 p.m. usually taken). Motions of same character, generally two or three in twenty-four hours. Since March 15th, the temperature has been falling gradually; this morning (March 19th), it was normal for the first time. The first day on which I detected abdominal symptoms, I ordered a simple linseed poultice, which, curiously enough, raised three small blisters over the site of the first three spots I had noticed; they enlarged considerably and ran together, forming an unpleasant

sloughy sore. The mother, who applied the poultice herself, assured me of its not being too hot. I have not as yet traced the origin of the disease.—*British Medical Journal*, April 5, 1879, p. 506.

9.—NOTES ON TREATMENT OF DIPHTHERIA.

By Dr. T. M. LOWNDS, late Professor of Anatomy and Physiology, Grant Medical College, Bombay.

As diphtheria has lately occupied considerable attention, I may be permitted to narrate the treatment I have pursued for the last eleven years with almost unbroken success. Looking, with the late Dr. Jas. Begbie, on the disease as allied to erysipelas, I have treated it with one of the persalts of iron, and I may say that, however successful the tincture of perchloride of iron has been in erysipelas, I believe the form of the persalt that I have used is quite as efficacious in diphtheria. My cases of the disease have occurred at all ages, from under one year up to seventy-six, the last being that of my oldest patient. The fatal case (seen in consultation) was one immediately following scarlatina, and it proved fatal in two days.

A large number naturally comprises a vast proportion of cases of slight severity, some of which would probably have done well under any treatment which did not lessen the powers of life; but upwards of twenty were cases very severe, if judged by the great depression of the vital powers, the dusky hue of the countenance, the deep redness of the fauces where the exudation did not conceal it, and, lastly, the appearance of the characteristic of the disease—the throat exudation.

The general form of prescription, which I have used invariably, modifying dose and frequency of administration according to the age of the patient and the severity of the disease, is as follows: Tincture of perchloride of iron, three to four drachms; solution of acetate of ammonia, an ounce and a half to two ounces; chlorate of potash, one drachm to a drachm and a half; water to eight ounces. It must be observed that in this mixture the perchloride of iron is converted into the peracetate, and there is also contained a portion of muriate of ammonia. There is also chlorine liberated, at least it is so when the tincture or solution is added to chlorate of potash; but as these notes have reference solely to treatment, I need not refer further to the changes which take place. This mixture I have used as frequently in the worst cases as a tablespoonful every hour, during the first day or days of treatment. I have never interfered with the sloughing exudation in any way, only prescribing a gargle or wash containing half a grain of permanganate of potash in an ounce of distilled water, to be used

frequently. This may be applied with a sponge or a brush, or inhaled as atomised vapour. I have always tried to get one evacuation daily from the bowels, and have supported the powers of life by wine, soup, milk, &c., as each case seemed to demand. How free such support has occasionally been, one of the cases outlined will show. Lastly, I have always insisted on perfect ventilation of the bedroom by a window opened at the top, so as to insure fresh and pure air to diphtheritic sufferers.

I now append outlines of a few cases.

Miss A——, aged about twenty-one, sent for me, June 25th, 1872, having for three days felt very ill, shivering, aching all over the body; had headache, and for the last two days she had kept her bed with, as she expressed it, a very bad sore-throat. She had used the permanganate gargle, which a former patient had recommended. I saw her at 2 p.m. In addition to what I have said, she could scarce raise or turn her head to the window for my inspection of the throat, and she could not raise herself in bed without feeling very faint. Both tonsils, especially the left, covered with ragged dirty-whitish patches. Pulse 118, very small and compressible; bowels opened; skin dry, 103·8°; countenance oppressed, and very dusky; her tongue was brown from the use of the gargle. In addition to the gargle, I ordered half an ounce of peracetate mixture every two hours; wine and soup. On June 25th, at 9 p.m., the temperature was 101·4°; pulse 106. On the 26th, at 8 a.m., the temperature had fallen to 99·6°, and the pulse to 94. There had been profuse perspiration in the latter part of the night. The heat of the skin never returned in this case, and though she was very weak, the throat was quite clear from sloughs by the fourth day (June 29th), and she made a perfect though tedious recovery. She had taken eight ounces of the mixture in the first eighteen hours. There was never any albumen found when I examined the urine. In the lodgings where this young lady resided there was a very foul smell from the drain in the back kitchen.

A girl, aged about thirteen, slept over a stable. In the corner of her room was a sink, which communicated, without a trap, with the main drain. At times this smelt exceedingly bad. On June 1st, 1872, I was called to see her. She had all the usual symptoms, and her throat, tonsils, uvula, and pharynx, were all covered, except one deep red patch, as large as the point of the little finger, on the pharynx. Pulse 114; temperature 102·8°. Ordered hourly doses of the mixture, with the gargle as frequently, and to have hourly doses of wine and beef tea in small quantity. After two days, the exudation began to curl up at the edges, and she gradually improved until, on the 8th, the throat was free from any speck. Her convalescence was

slow, as she frequently suffered from fainting fits, which, I was told, lasted as long as an hour or more. She did not lose these faints until six weeks after the invasion of the disease. I think that this was the most severe case of diphtheria uncomplicated that I have seen.

In one other case the exudation was not absent from the throat until seven days and a half had elapsed. This was in a lady aged forty-five, and it was followed by paralysis of the muscles of the throat and weakness of the muscles of the limbs, which did not disappear for six months.

Of the next case, that of a girl aged seven years, I give only a bare outline, as it extended over three months and a half. In October, 1873, she had an attack of measles, from which she was scarcely convalescent when her sister suffered from scarlatina in a severe form. She also took the infection, and the eruption in her case rather presented the appearance of r  theln, but it was evidently the same disease from which two brothers and her sister had suffered, and in an aggravated form, and to be succeeded, before she left her bed, by diphtheria, which at once, on November 2nd, covered tonsils and the whole of the visible front of the throat. She had all the signs and symptoms of the worst form, and the glands of her neck were all much swollen, so that she could scarcely swallow. Her temperature was high, though I cannot find my notes on the point; pulse 120, very weak. She could only take the mixture five or six times a day, and could but wash the mouth with the gargle. She could not take food, so about the 3rd or 4th November I ordered enemata of Liebig's raw soup mixed with an equal quantity of port wine, to be given in two-ounce doses (of the mixed liquids) every hour. The diphtheria extended at last to the upper part of the larynx, and produced a suffocative cough. For eighteen or nineteen days this treatment was continued; as much nourishment, with at times a little champagne or brandy and water, as she could swallow, was given. Her mouth was washed out frequently, and for some days she coughed up stringy membrane, which was drawn out of the mouth by her mother. I examined the urine frequently; albumen was never found. For these days nearly a bottle of port wine a day was given by enema (six bottles a week), and only on one occasion was the small two-ounce injection returned, and this was apparently caused by the addition of a small quantity of aromatic spirit of ammonia, which the rectum would not tolerate. During the whole time that this rectal nutrition was carried on, there was usually one formed motion in the twenty-four hours. Owing to her emaciation, she had, after a week's illness, been placed on a water-bed. The laryngeal diphtheria had scarce disappeared

when she was attacked with severe articular rheumatism, and it was ten weeks before she left her bed, with contracted lower extremities. However, galvanism and friction gradually restored the use of her limbs. After she was well, she suffered from what her mother called nervousness for nearly two years.

In 1874, in October, November, and December, I treated fourteen or fifteen slighter cases of diphtheria, in which all the throat mischief seemed at once subdued (*i.e.* in one or two days); but I then noticed for the first time how, in early convalescence, assuming the erect posture caused the pulse at once to spring from 64 to 72, when sitting, to 115, and in one case to 130, when standing. When I first observed this, I thought I must have made a mistake, and I repeated the observations with the same results.

I instance one more case. J——, a boy aged fourteen, was on Feb. 5th, 1879, convalescent, and nearly well except for a slight throat cough from an attack of influenza. His attendant then observed that he was very hot, had a sore throat, headache, foul breath, and that he was very weak and languid. The bowels were constipated, and a dose of aperient medicine was given. On Feb. 6th I saw him. The same symptoms were all present, and he had diphtheritic patches on each tonsil, that on the left being largest. All around the fauces was a dusky deep redness. Great headache and giddiness, so that he could scarce stand for one moment without support. Bowels not opened. Temperature 103.3° ; pulse 116. Ordered a dose of castor oil, the ordinary mixture and gargle, and soup and wine. On Feb. 6th, the bowels having been freely opened, the temperature was 104.3° ; pulse 108. The patient continued very restless and hot till 4 a.m. on the 7th, when perspiration set in, and he slept till seven o'clock quietly. Bowels still purged. Temperature 100.3° ; pulse 94. Patches in throat clearing off at edges. He got better towards evening, and the dusky hue had disappeared. Temperature 100.4° ; pulse 94. On the 8th the temperature had fallen to 97.8° , and the pulse to 58. Bowels still irritable. Patches nearly gone. To have bismuth with small doses of morphia before meals. On the 9th his appetite had returned. On the 10th the pulse was 74; he had good strength, and felt nearly well. On the 18th well, but still weak.

I have not traced the treatment beyond the stage of exudation, as then all cases, I believe, require the general treatment applicable to the prominent symptoms and the particular state of each patient.

I need scarcely say that with such cases as I have mentioned I place much dependence on this combination in the treatment of diphtheria. I could mention many more similar cases. The

steel mixture has not the unpleasant taste of a mere dilution of tincture of perchloride of iron, and young patients do not object to it. The original reason of the combination was to render it less nauseous. I have also found it as effectual certainly, nay, I think, more so, in treating erysipelas, than the simple mixture.—*Lancet*, March 22, 1879, p. 403.

10.—ON SULPHUR AS A TOPICAL APPLICATION IN DIPHTHERIA.

By JOHN A. ERSKINE STUART, Esq., Dunse, N.B.

On January 6th, 1879, I was called to the country to see a boy, J. P., aged eight years, who had been complaining since the 3rd of the month of a sore throat. I found him complaining of a bad headache, and of a severe pain shooting out at his right ear. On examining his throat, I found the tonsils much swollen, and of that peculiar deep red colour so characteristic of the disease, and according to some writers indicative of the disease although no membranes are present. On the inner side of the right tonsil was a patch of membrane, three-fourths of an inch long and one-fourth broad, covering an excavation in the tonsil; on the left tonsil, on its inner side, was another patch of about the size and shape of a sixpence. The sub-maxillary glands were very much enlarged on both sides. His face and lips were much congested at times, his colour rapidly changing. His pulse was 84, and very weak and intermittent. I at once procured some sulphur and a quill, and blew a considerable quantity on to each patch of membrane.

Next day, his pulse was similar, his temperature 100°. There was no albumen in his urine. His mother continued, as ordered, blowing the sulphur on to the diphtheritic patches every hour. On the previous evening he was vomiting for a considerable time, and subsequently on examining his throat I found the right patch entirely absent, except a piece about the size of a pin-point. Considerable shreds were detected in the vomited matters. The left patch had considerably increased, but the next day, by perseveringly directing the sulphur to that side in particular, it was reduced to one-half its former size, while on the next visit, two days after, the throat was clear of membranes, although still slightly red. The patient was sitting up by the fire on that day, which was exactly one week from the day he first complained of his throat. One case is not of much use, but perhaps others may be tempted to give the sulphur a fair trial, and report progress. In this hope, I have written these notes. That sulphur does blacken the membrane there is no doubt, as I demonstrated for myself in this case. How it

detaches the newly-formed exudation (unless by killing it), is a puzzle wanting an answer.

Although the diphtheritic membrane was so quickly dissipated in this case, yet the patient suffered afterwards from a slight attack of paralysis of the larynx and pharynx. Shortly afterwards his elder brother took diphtheria, but the mother by means of the sulphur treatment cured him. A case of this disease, worse than the recorded one, was successfully treated in the above manner by me, about a fortnight ago. In this case, however, swabbing sulphur and water on the membrane took the place of blowing it on.—*Practitioner*, April, 1879, p. 248.

11.—ON SOME VARIETIES OF SORE THROAT.

By Dr. R. L. BOWLES, Folkestone.

Lately, a friend of mine at a distance pronounced a case one of inflammatory sore-throat; a few days later, the same case came under my care as quinsy. On another occasion, my friend saw a case of what he again called inflammatory sore-throat, which this time turned out to be what is commonly named an ulcerated throat of the diphtheritic kind.

For these reasons, and from the fact of finding many of my medical friends in the same state of confusion as I am myself, I relate the following cases somewhat minutely, as they are good types of two forms of sore-throat.

Case 1.—A lady went to bed feeling indisposed; she had a bad night from fever, headache, pains in the limbs, and sore-throat. In the morning she was very ill, and I found a deposit, looking already like a large loose slough, on each tonsil. The tonsils were somewhat enlarged, of a very deep red colour, soft looking, with loose folds of tumid and oedematous mucous membrane. The next day in the evening, the slough had gone, and the tonsils looked ulcerated; but, in consequence of the folds of mucous membrane, it was not possible for me to be absolutely certain on this point. The illness lasted three days, and left her very weak. The husband had suffered from a precisely similar throat four or five days previously, and a maid and a sister some days before them. The gentleman and lady had been visiting where the sister and maid had just suffered, and where there were bad smells in consequence of the frost having stopped the water supply.

Case 2.—On February 6th, I was called to see my boy at school in Folkestone; he had passed a very bad night, with soreness and dryness of the throat, and was now suffering from weakness and a dry husky throat, such as is found at the commencement of laryngitis. He had gone to bed the preceding

night, as far as he knew, perfectly well, and wholly unconscious of approaching illness. I found the lymphatic glands of the neck swollen and tender, with general infiltration of the cellular tissue; the tonsils were enlarged, of a pinkish red colour, smooth and dense looking, whilst from twelve to twenty white spots were dotted about at pretty even distances upon their surfaces. These spots were elevated like commencing small-pox pimples, and were evidently covered by the thin transparent, almost unaltered, mucous membrane. One or two of the spots appeared to be exuding their contents; the soft palate and uvula were tumid and inflamed. The boy looked white, with glassy eyes, and felt evidently very ill; his pulse was quick, and skin dry.

He was conveyed home, and a dose of castor oil was given, and subsequently at intervals doses of liquor ammoniæ acetatis and tinctura aconiti were administered. In the evening, he was somewhat excited in manner, and had a great craving for food. Temperature 101·2 deg.

February 7th. He had passed another most uncomfortable night. Pulse 108; temperature 101·2 deg. The neck was still as swollen, and the tonsils were scarcely altered, except that many of the white spots upon them were now open, and they looked somewhat larger (from the size of a large pin's head to that of a grain of linseed). It was necessary to repeat the castor-oil, after which the boy felt very sick, and would take nothing all day, partly from nausea produced by the oil, partly from feeling so much soreness of throat. He had two very foul evacuations after the oil had been given; and at night, although he felt no better, his throat inside was rather of a paler hue; some of the spots had gone, but others were larger; the lymphatic glands were more tender. Pulse 116; temperature 102·4 deg.

On February 8th, although he had had a very wretched night, with short snoring sleeps and throat-distress, feverish restlessness and headache, the pulse had gone down to 100, and temperature 100·8 deg. The cellular tissue of the neck was less distended, but the glands were very sore and tender; the spots inside were larger and tending to coalesce, and, at the bottom, close to the root of the tongue, were two membranous patches, nearly as large as a threepenny-piece; from a very careful inspection, I formed the opinion that these patches were coalesced follicular deposits flattened out by the pressure of the base of the tongue. He had subsequently a loose action of the bowels, with the passage of a large quantity of free bile.

February 9th. On the previous evening, he had been again worse, and had afterwards a trying night; but this morning his temperature was only 99·4 deg., and pulse 86. The

follicular deposits were fewer in number, and the larger patches below were breaking up; but there was a thin transparent film surrounding the uvula. His urine, which deposited lithates largely, was of specific gravity 1040, free from albumen and sugar.

From this time, he gradually convalesced, but remained weak and white for a fortnight. He was isolated for eight days, and no other case occurred.

Five or six other boys in the school had troublesome colds about this time; but the weather was so bad that this was not surprising. A week or two previously, the water-supply to the closets was deficient in consequence of the frost, and the town drain had been open a hundred yards away, but there was no palpable unsanitary condition in the house.

Change the names, and, *mutatis mutandis*, the beginning of this case was precisely parallel with the cases of two children formerly reported to this society, both of whom were attacked at the same time, and as suddenly; the state of the throats in both cases could not have been distinguished from this one, nor from each other; the swollen cellular tissue and lymphatic glands were alike, and the general disturbance of the nervous system corresponded. These two children had been out in the country together one or two days previously, but lived in different houses, and were otherwise under different circumstances. From one of these, my eldest child took diphtheria as severely and in as well marked a form as I ever wish to see, and every one in the house who was allowed to enter her room had milder forms of the same disease; whereas those in the same house and otherwise under similar circumstances who were not admitted into the room were wholly unaffected; so it seemed probable that it could not have been the house which was in fault.

On the other hand, no one in the house where the other child was ill suffered in any way.

On the same day this last February that my boy was attacked, another boy aged eight, in a different part of the town, also complained of not feeling well, and of sore-throat; and, when I saw him two days later, he had swollen lymphatic glands and swollen tonsils with follicular deposits in front, and some membranous-looking patches as large as a sixpence below and behind and on the back of the pharynx. Two days later, the tonsils were quite free from membrane and reduced in size, but he had a sharp cold in the head, and subsequently cough with fine rhonchus. The case was isolated for ten days, and no one in the same house caught the disease.

Now, it always is of very great importance, for many reasons, to arrive at a correct diagnosis in these cases; for if it be of the

infectious variety, all the anxious questions connected with the details of isolation come thick upon us, and must be dealt with; but, if it be catarrhal only, we do harm by treating it with iron perchloride and such like remedies; possibly—aye, indeed, probably—increasing the fever and taking away the last chance of preventing the formation of that most painful and miserable complaint, a tonsillar abscess.

Of course, a typical case of diphtheria affecting the fauces we should all at once recognise, and deal with in the typical fashion; but, unfortunately, in nature the exceptions to types are so common, that we must ever be in a frame of mind to believe almost anything possible.

In quinsy, I believe, the original inflammation, with its consequent abscess, always arises in the structures *behind* rather than *in* the tonsils. The excessive pain of using the muscles of deglutition in the very beginning of the attack, when often there is very little to be seen amiss with the tonsil itself, the deep-seated soreness, and its frequent connection with rheumatism and gout, suggest muscular and fibrous tissue-origin, and the nature of the pain when the muscles are in action puts one forcibly in mind of stiff-neck and lumbago.

In the three cases (taking quinsy as the third) now described, I only proposed to myself to define accurately the anatomico-pathological conditions which came under my observation, and to urge that it is of some importance to form an accurate judgment in the early stage of a case of sore-throat, in order that the patient's family, as well as the patient himself, may know what to expect and what to do. These three conditions may be compared to the various ophthalmic inflammations: the first, where there is conjunctivitis with chemosis and sometimes sloughing of the cornea; the second, where the sclerotic and deeper structures are invaded; and the third, a post-ocular inflammation with possible consequent abscess.

Of course, there may be modifications and combinations of these conditions in both throat and eye inflammations; but the greater prominence of one or other will be a partial guide to us in our prognosis and our treatment. I am not desirous, on the present occasion, of dealing with anything but facts; but I cannot help hazarding the strong opinion I entertain, that the first class of cases, in which the mucous membrane is specially attacked, and a large loose slough rapidly forms, is more particularly connected with bad drains, and that it is not *true* diphtheria, although I am quite prepared to find that diphtheria may supervene upon this, as well as upon any other inflammatory affection of the throat.

Since writing the above, I have read with much interest the paper on what he named 'Spreading Quinsy,' by Dr. Cornelius

Fox, in the *British Medical Journal* of December 7th, as well as all the letters and notes which have subsequently appeared, and which have chiefly been called forth by that paper; and I am bound to say, it is most unsatisfactory to find such utter confusion prevailing on a subject upon which, perhaps almost more than any other, we are called to guide and advise our anxious patients.

To me it appears impossible to be sure, on first seeing a case of sore-throat, whether it will be diphtheria or not, even indeed if there should be false membrane. It is now almost certain that some low organism has an influence over the origin and propagation of true diphtheria, and it is very probable that these organisms only find a suitable soil in lowly vitalised or perhaps decayed animal matter, in a manner similar to the mycelium in the thrush of infants enfeebled by illness or want of care. So, in local inflammations, especially of the throat (whether from cold, chill, or other cause), the organism of diphtheria may readily find a suitable soil in the thrown-off epithelium and decomposing exudations of the inflamed mucous membrane.

We should, therefore, be careful and tentative in the opinion we give on first seeing a case of sore-throat. Still, I am full of hope that if we all steadily set to work to observe carefully, to define our cases clearly, and at the same time bring into prominent relief all the attendant circumstances, we shall not be long before we bring order out of chaos, add another claim to medicine as one of the exact sciences, and so enable it more adequately to fulfil its mission of rendering aid to suffering humanity.—*British Medical Journal*, April 5, 1879, p. 503.

12.—NOTE ON DIPHTHERIA-DIFFUSION.

By Dr. ARTHUR DOWNES, S.Sc. Cert. Camb., Shrewsbury.

From investigation of numerous outbreaks of diphtheria in distinct localities, I have long been led to the conclusion that the dissemination of this disease in rural districts is mainly dependent on the numerous mild *unrecognised* cases, which may generally be discovered about the neighbourhood to which attention has been directed by an appearance of the disease in its severer form. In this dissemination some day-school usually plays a prominent part.

Others notice the existence of these slight cases in infected localities, but have regarded them as primarily non-infectious, acquiring specific diphtheritic infectivity by transmission through successive subjects. I am not aware in what way they consider that these “non-infectious” sore-throats are in the first instance transmitted from person to person so as to acquire this quality

of specific infectivity; my own experience, indeed, is that they are simply cases of true diphtheria, so mild in degree that the symptoms, both local and general, are in abeyance, and pass almost unnoticed. The proof of their true character is that under suitable conditions their contagium gives rise to typical diphtheria, and, on the other hand, that they themselves may often be traced to an antecedent case of unmistakable diphtheria. They are decidedly infectious; not so highly infectious, perhaps, in a relative sense, as are the typical and malignant forms, but in the aggregate far more to be dreaded than these as foci of diffusion. As a rule, no medical advice is sought by the patient, who goes about, or attends school, as usual. In sickness returns, however, I have seen these cases returned not only as "sore-throat," but also as "bronchitis," "cold," &c., and, indeed, they are, I believe, *per se*, and without a knowledge of their history, not specifically diagnosable. They generally far out-number the well-marked typical cases.

A chief feature of diphtheria, noted by most observers, is its association with cold and damp. Damp, whether arising from defects of subsoil, surroundings, or of house construction, is a constant characteristic of those endemic haunts of diphtheria with which I am acquainted. Without prolonging this paper by a discussion of the various views that have been put forward as to the nature of the diphtheria contagium, the supposition may reasonably be entertained, and supported by evidence, that the specific virus may be associated with some low form of *micrococcus*. From the persistency with which the infection hangs about particular spots—which my experience leads me to regard as the starting-points and foci of epidemics—it would appear not improbable that such an organism may survive and develop externally to, and independently of, the human body, spreading, perhaps, as a minute growth over congenial damp surfaces; nor would it be impossible that the influence of cold and damp upon its activity might be analogous to the effect of those agencies upon some low algaoid forms, whose growth appears to be promoted under such conditions. Be this as it may, it is a most important point in practice, to see that after convalescence not only clothing but also floors and wall surfaces are thoroughly disinfected and cleansed.

Diphtheria may, I believe, be occasionally conveyed in clothing, and I am personally acquainted with several cases in which the contagion has apparently been carried home from infected houses by parents and others who have themselves shown no symptoms. In none of the epidemics of which I have notes has any relation of the march of the disease to water or to milk-supply been found. As regards milk, however, it must be observed that, from the difficulty of obtaining it, milk

enters less into the dietary of most country poor than of their brethren of the town. I should expect, therefore, that if any relation exists between diphtheria-diffusion and milk-supply, it would probably be more demonstrable in an urban outbreak than in a rural. In the country, moreover, filth nuisances, while they appear to determine the localisation of the severer types, and to be occasionally an important factor in the causes of mortality, have not, in my experience, any direct influence on the extension of the epidemic.

These experiences relate only to diphtheria as it occurs in purely rural districts. Its incidence on these, as compared with the general immunity of towns, is one of the most striking features in the natural history of this disease, and in which it stands in bold contrast to most other zymotics. Thus, while of a million children born in the "healthy district" of the Registrar-General, 10,292 may be expected to die of diphtheria, in Liverpool the corresponding proportion is only 4416. Yet the total death-rate from all causes in the "healthy districts" averages only 16 per 1,000 annually, and the density of population in persons to a square mile is but 166; while the death-rate of the Liverpool district (1861-70) averaged 39, and the density was 65,823 persons to the square mile. Without pretending to attempt the solution of this problem, it may be interesting to note incidentally the effect of town air on certain low forms of vegetation. For example, the comparative absence of lichens from towns generally, and their exuberant growth in pure country air, is a well-observed fact; and an eminent botanist recently informed me that he had on this account regarded these plants as a rough measure of the healthfulness of a locality. We may conceive that there may be some constituent of a town atmosphere—perhaps in the smoke—present under ordinary circumstances in such proportion as to be as inimical to the growth of a *micrococcus diphthericus* as to the development of a lichen.—*Lancet*, April 12, 1879, p. 515.

13.—OBSERVATIONS ON ACUTE RHEUMATISM.

By Dr. REGINALD SOUTHEY, Physician to St. Bartholomew's Hospital.

My observations have taught me that both for prognosis and towards estimating correctly the value of any particular treatment, or of physiological rest and no treatment at all, it is advisable to distinguish clinically between the acute continued and the acute relapsing varieties of rheumatism. Both are affections of youth and early manhood, rather than of childhood, middle or old age.

The acute continued form attacks the muscular and robust;

it presents, doubtless, an abatement of suffering when the individual is in bed, and the joints are freed so far as they can be from muscular strain upon them; but in it we seldom observe, except from the action of sedatives, any marked remission of pain or of fever before the 8th or 9th day of the disease; up to this date the temperature chart is a pretty steadily maintained one—at 101.5° or 102° (barring pericarditis). The articular inflammations then subside suddenly and the temperature slowly de-fer-vesces, the patient usually being fit to go about again about the 21st day from that of first invasion.

The cases published by Sir W. Gull and Dr. Sutton in the *Medico-Chirurgical Transactions*, vol. lii. p. 43, ably testify to the natural proclivity in this form of the disease for the acute symptoms to subside from causes wholly irrespective of treatment between the 8th and 10th days, when, as they also notice, the patients usually convalesce gradually and without drawback, getting up for the first time on or about the 19th day, and being strong enough to be discharged from the hospital a few days later.

The observations of Dr. Sibson, published in an article communicated to the *British Medical Journal*, August 13, 1870, led him to the conclusion that in a fourth of all cases the joint affection lasted over twenty-one days, and that half of his cases presented relapses.

Dr. Wade, in his *Notes on Clinical Medicine*, Birmingham, 1864, mentions the tendency of acute rheumatism to relapse. "Out of 109 cases," he writes, "25 relapsed, some several times."

Dr. Da Costa, in the *Pennsylvania Hospital Reports*, gives analyses of 30 cases treated by ammonium bromide, which he thought diminished the severity of his cases and the tendency to relapse—circumstances, however, which his statistics scarcely support.

My observations teach me that proportionally more relapsing cases occur in winter than during other seasons; that they principally affect anæmic cachectic individuals, whose health has suffered.

What is quite characteristic of this variety is its apparent yielding to any treatment which is adopted. Once rested in bed, the patient sleeps without sedatives, and loses nearly all his acute pain upon the 7th day of the disease. The temperature falls to normal in a large proportion of these cases; the patient then desires more food, wants to get up, and often does so. After three days or more, often upon the 3rd day, back come the articular pains, and the fever lasts for varying periods—two, three, or seven days, but seldom beyond this; for on the 14th day, in far too large a proportion of cases to be accidental, and

too independently of any particular treatment for the circumstance to be due to this, the pains and fever again somewhat suddenly remit. This remission may be permanent, and usually is a longer remission than the first, but it may be succeeded by a second, a third, or even a fourth relapse.

Experience has taught me that each relapse of the disease, instead of proving milder, is usually, and especially in the cases treated by alkalies or iron, more severe; the fever runs higher and the articular pains last longer, while very few of the cases which have two relapses fail to present cardiac murmurs and permanent valvular lesions.

Treatment has appeared to me to exert no inconsiderable influence upon those relapsing cases, whether directed to relieving pain or preventing relapses. Large doses of quinine, administered every other night during the remission, has seemed in my hands, quite lately, to obviate relapses, or to reduce their length and severity in no small degree.

Almost every practitioner has his own pet treatment for rheumatism, and, just as with particular religionists, seeks usually to proselytise according to the fulness of his faith and the degree of his zeal; sure proof, as Sir Thomas Watson has truly said, of the obstinacy of the complaint and the lack of any absolute specific for it. If my experience does not enable me to speak quite as highly in commendation of salicylate of soda as my friend Dr. J. Pollock and some others have done, who have been singularly fortunate with this remedy, or less tutored by statistical investigation of the disease otherwise treated than I have been, I am still prepared to allow this much, that it has in my hands never caused any more than temporary inconvenience, and has always appeared to clean the tongue and to relieve the articular inflammations when administered in sufficient doses.

The following indications for treatment have guided me in my practice upon acute rheumatism for some years, and I give them for the help of younger practitioners:—

To relieve the local pains; to reduce the fever; to diminish the gravity of the various complications most usually incidental; to promote early and complete convalescence.

I will not waste time in any speculations upon the materies morbi, nor the mode of its elimination; it is certainly not eliminated by either sweating or purging.

A spring-bed, with a tightly-stuffed horse-hair mattress over it, is that which rests and supports the trunk and limbs and relieves the joints most, and what I recommend for my patients.

Painting the affected joints in the acute continued form with a mixture of equal parts linimentum iodi and tincture of iodine

has appeared to me to procure all the benefit advocated by Dr. Davies for his blister treatment, without producing the unnecessary pain of open blister wounds, or risking the production of acute nephritis or cystitis. The linimentum alone is for most skins too strong; the tincture of our Pharmacopœia for nearly all useful purposes too weak.

I am no advocate for, and have never allowed, my patients to be placed between the blankets instead of the sheets; neither will I permit flannel bed-shirts to be worn. The object I have in view is to keep one uniform temperature about them, which the bed provides for, but not to keep the patient swathed in flannel reeking and saturated with decomposing perspiration. As a matter of fact, I observed that a large proportion of hospital rheumatic patients were persons who habitually wore thick flannel shirts and undershirts, and did not change them very often. Cotton shirts and cotton sheetings are better suited than linen to rheumatic people.

My practice has been to keep my rheumatic cases cool rather than hot; they are certainly easier in consequence. As to baths, a tepid wash-bath at 98° or 96°, into which they can be dropped in a sheet and lifted similarly back into bed, comforts them and secures their cleanliness.

I will not go so far as to say that purgatives are to be entirely avoided in acute rheumatism; but the old routine practice of calomel and jalap, because a patient's tongue is furred and his bowels have not acted for twenty-four hours, is, in my opinion, the crudest medicine. I occasionally order a dose of castor-oil or of Carlsbad salts if the abdomen is full, but more frequently employ simple enemata than purgatives to unload the bowels.

The alkaline treatment or peppermint water is equally well adapted for the acute continued forms; but it appears to me that alkalies, unless pushed to the full and frequent doses first urged by their original proposers, are wholly useless in relieving pain, while, when they are given in half-drachm or drachm doses of bicarbonate of potash every two or three hours, they quickly reduce the strength and render the patient extremely anæmic. They appeared to me, thus employed, to diminish, perhaps prevent pain, but to retard convalescence not considerably.

Salicylate of soda appears to lower the pulse, to diminish the temperature, and to relieve the articular pains; but to secure these effects it has to be given in moderately large and frequent doses. I usually give 15-grain doses of it every two hours until 60 grains have been taken, and then keep up the effect of the drug with 15-grain doses every four hours.

If pericarditis is present, I usually give small doses—five drops—of liq. opii. sedativ. with each dose of the salicylate.

My experience of the perchloride of iron treatment, as recommended by Dr. Russell Reynolds, is not favourable to the employment of this drug at all. In the acute continued cases it has been associated too frequently with hyperpyrexia for me to regard it as altogether free from danger; and in the relapsing forms I found its administration neither shortened the cases nor prevented relapses.

The relapsing forms may be kept upon salicylate of soda very well for the first seven days; but the opportunity of the spontaneous remission which then generally takes place, and is indicated by the thermometer and the patient's own sensations, should be seized to shake the periodicity of the complaint. This quinine, I think, has the power of doing, given in the proper doses, as in ague, and at the proper date. I give two 10-grain doses at two hours' interval, between 5 and 7 p.m. on the evening of the 7th day, and repeat the dose on the evening of the 9th day again.

The natural relapse is then postponed, and the patient may be permitted better nourishment, and given a little pepsine and acid with his dinner to ensure its better digestion. Should no relapse happen, again on the 14th and 16th days full doses of quinine should be given; but it is not until the 32nd day is past that all risk of a relapse is over. During convalescence, although the patients are anæmic, I seldom recommend iron; the mineral acids and tincture of nux vomica promote appetite and digestion far better. Chloroform and belladonna liniment, as recommended by the late Dr. Sibson, is an excellent pain-relieving local application in either the acute or relapsing form of the disease, if the patients are hyper-sensitive to pain, or the articular affection is inordinately severe.

Quinine and iodide of potassium has long been a favourite draught of mine in treating the anæmic relapsing forms. Two grains of quinine are combined in it with five of iodide of potash; and this draught I generally order to be taken after the 14th day every four or six hours.—*St. Bartholomew's Hospital Reports*, vol. xiv., 1878, p. 22.

14.—THE OMISSION OF SODA-SALTS AND ALCOHOL IN THE TREATMENT OF GOUT AND RHEUMATIC GOUT.

By Dr. JOSEPH DREW, Beckenham.

The following observations in the treatment of gout and rheumatic gout are given by one who has suffered much during the past twenty years from these diseases; and since, by the measures adopted, he has been free from any attack for more than a year, not to mention the great improvement in general health both of mind and body, he hopes some of the readers of

the British Medical Journal may be induced to test on their patients, or on themselves if necessary, the decided efficacy of the treatment recommended.

The writer, born of parents free from gout, was seized with his first attack in the right ankle-joint at the age of thirty, and then suffered recurrences, at unequal intervals, in the right or left great toe and foot. As time passed on, the disease returned more frequently, until in some seasons it appeared as though the *materies morbi* was formed as fast as it was eliminated; and it became evident that a constitution otherwise healthy, although perhaps never one of the strongest, was fast losing its powers of life.

It will not be necessary to give the preceding and accompanying symptoms of each paroxysm, as they have been so truly and eloquently described by Watson and other classical medical writers; but one sign is unique according to the reporter's experience, and probably deserves a note in the history of this troublesome complaint. As soon as an attack became imminent, sudden congestion of the glottis and larynx occurred, with necessarily difficult breathing. If sleep supervened only for a few minutes, inspiration became so noisy (chiefly whistling) that it awoke the patient and alarmed the attendants. This disagreeable symptom continued until local inflammation was established in either foot, and the mucous membrane, relieved by derivation, was allowed to resume its proper function.

As in most cases of gout, rheumatism soon came to assist in the crippling process; the hands became so stiff and the joints so prominent, that the fist could with difficulty be closed, or a firm grip given. There was not only interstitial extravasation of urate of soda, but also considerable thickening of the phalangeal bones from repeated periostitis.

Neither will it be necessary to report all the remedies taken during this long period, for a man who can truly say, "*in hâc re, plus scio quam vellem*," would be sure to try every agent that had ever been recommended or suggested.

And now the interest of the case begins, for the resolution was taken to give up completely the use of every alcoholic beverage. In former years, malt liquor had been relinquished, and with good results for a time; then wines too were added to the list; and last of all, in August, 1877, spirits were suddenly and entirely banished. After a week or two of purgatory, the good work began, and has continued without intermission up to the present moment; so that the patient not only has been free from any attack of gout, but has felt and still feels better in himself than he has been for the long period of twenty years or more.

After waiting patiently for all that recuperation which the

vis insita naturæ was able to accomplish, there still remained the stiffness of the hands and the periosteal thickening of the phalanges, which nature, unassisted, could not entirely overcome. Suddenly, about five weeks ago, whilst eating a morsel bountifully seasoned with salt, the thought occurred, "Can chloride of sodium be right for a gouty system, when cakes or biscuits or tea made with soda, or even soda-water (when containing that alkali), will produce an attack of this disease?" Certainly not, theoretically; for an atom of oxygen united with the sodium will present to the uric acid of the blood the worst poison it could possibly encounter.

The idea once started, it was, of course, immediately carried into practice, and chloride of sodium was placed in the *Index Expurgatorius*. Salt was omitted as an article of diet, not merely as a condiment, but avoided in salted meat or any other suspected comestible. The result in four or five weeks has been astonishing: most of the stiffness has passed away; rings that had been laid aside can be reworn; and the phalangeal bones have almost returned to their primitive size and shape.

If further observation should prove the truth of these personal experiments, a great advance will be made in the treatment of gout, rheumatic gout, and perhaps also rheumatism; for the permanent cure of such patients will be ensured by eliminating, as far as possible, all articles of diet into the composition of which soda-salts enter in any considerable quantity.

The theoretical action of alcohol in the production of uric acid and gout is more clearly understood than the extraordinary effects of the omission of a few grains of salt; for the former interferes with function, whilst the latter is so universally spread throughout the body that it appears to be one of its most healthy constituents. Still, as certain effects have been carefully noted and felt by the writer, he deems it a duty to make them as patent and reasonable as possible.

In birds, reptiles, serpents, &c., the unnecessarily ingested and effete azotised matters of the body are got rid of in the form of uric acid and its salts, for in these creatures there are no long urinary tracts to be either blocked up or inflamed by these comparatively insoluble substances; but as we rise in the scale of being matters are not so simple, and a complicated secretory apparatus is found, consisting of kidneys, ureters, bladder, and excretory ducts; and it has been wisely ordered that the nitrogenous *débris* which, in the form of uric acid, would doubtless be dangerous to life, shall suffer further oxidation and become converted into the more soluble urea. Any agent, therefore, that interferes with this oxidising process, such as cold, nervous prostration, excessive nitrogenous diet, over-

exertion, or alcohol, will, in the gouty diathesis, produce gout, and in different constitutions other diseases, which need not now be particularly specified.

The injurious effects of salt in a gouty habit of body may be accounted for in two ways: either by its interfering with blood-formation, and so indirectly preventing the circulation of oxygen, necessary for the conversion of uric acid into urea; or, by its superabundance in the blood, compelling the formation of more urate of soda than can be conveniently got rid of by the ordinary channels of the body.

[Dr. ARTHUR LEARED, in a letter to the British Medical Journal, on Dr. Drew's paper, remarks:—]

Having a personal interest in gout, I wish to say a word on Dr. Drew's statements about the cure of the disease in his own case by relinquishing the use of alcoholic stimulants, and also of the sodium-salts. I have no doubt the first-named omission was the true *eureka* in the matter. My conviction is that the entire disuse of alcohol always breaks the force of the attacks, and in a fair proportion of cases virtually annuls the disease.

In prescribing alkalies for gout, it has long been a rule of practice to avoid soda, because of the less soluble compound which it forms with uric acid, compared with that formed by potash. But Dr. Drew writes: "Can chloride of sodium be right for a gouty system, when cakes or biscuit or tea made with soda, or even soda-water (when containing that alkali), will produce an attack of the disease? Certainly not, theoretically; for an atom of oxygen united with the sodium will present to the uric acid of the blood the worst poison it could possibly encounter." As for chloride of sodium, it is not certain that it is decomposed in the system; and, as regards soda, the statements that it is the worst poison that gouty blood could possibly encounter, and that it directly excites the gouty paroxysm, are new to me. It were to be wished that Dr. Drew had mentioned his authority for these allegations, or whether they are the outcome of his own observations. In no disease, owing to the fitful and uncertain way in which it attacks the patient, is the *post hoc* argument more likely to be applied in the case of futile remedies. I know at present one gentleman who is convinced that he keeps away his gout by eating apples, and another who is equally convinced that his safety lies in daily gulping down the yolk of a raw egg. We know how to estimate the value of such statements; but, although a statement in such a matter made by a medical man may seem improbable, one feels inclined to hear the grounds on which it has been made.

[In reply, Dr. DREW says :—]

More than thirty years ago, I formed the opinion, in common, I believe, with all my teachers and fellow-students at that time, that soda was to be avoided in the treatment of gout; and a subsequent experience of sixteen years in general practice assured me of its wisdom; but it was not until this intractable disease began to make serious inroads on my own health that the importance of carefully guarding against this alkali in every comestible and drinkable was gradually forced upon my attention.

In the history of my own case, the causes of each attack were generally evident; either taking too much of this or of that, or severe cold, or prostration from loss of rest or from overwork; but sometimes severe attacks would come on without any apparent cause, even after the system had been cleared by potash, iodide of potassium, and colchicum. It was after one of these unaccountable attacks that I remembered we had taken tea the night before at a friend's house, and I subsequently discovered that the beverage had been brewed with the aid of carbonate of soda. When the next unaccountable attack occurred after tea with a friend, the mode of manufacture was inquired after, and again and again it was discovered that the offending alkali had been used; but sometimes attacks came on when I had lived most carefully and had not been out of my own house, but when unfortunately I had eaten cakes or biscuits made by the cook; and now I was led to ask my wife whether soda was used in the kitchen, and quickly discovered that such was the fact.

That even soda-water (when containing that alkali) will cause the disease, the following case will sufficiently prove. A gentleman in Hertfordshire had been confined to his bedroom for nearly three months from constantly recurring gout. The judgment of his own medical attendant had been assisted by two physicians from London; but the disease would not be subdued; every remedy likely to do good had been prescribed, and the regimen recommended was faultless, with one exception: he had been allowed to drink soda-water *ad libitum*. Unfortunately, the patient was a thirsty soul, and drank four or five, or six bottles *per diem*, according to his fancy. He was told to omit this soda-water, and in less than a week he was out of doors.

Not long ago, I was requested by a young medical friend in the country to give an opinion on a case of obstinate gout, which, to my astonishment, he was endeavouring to cure with rhubarb and carbonate of soda; and was told by him, to my still greater astonishment, it was the ordinary prescription

where he had been educated. Scores of practitioners are now scattered all over the country, and doubtless have left this Alma Mater with the same impressions of the treatment of gout as my young friend.

The omission of chloride of sodium as an article of diet in the cure of gout is only a recent idea, and requires further trial: I do not wish any of your readers to suppose it is yet proved that this salt undergoes decomposition in the blood; all I wish to declare is that, in my own case, its omission has been productive of great good, and I can only account for this beneficial action by supposing that the uric acid in the circulation is compelled as it is formed to unite with other alkalies when such an abundant supply of soda-salt is suddenly cut off.

My experience in the treatment of others would lead me to believe that not every case is so susceptible to the influence of soda as to suffer an attack of gout from the small quantities sufficient in my own system, but only in those constitutions with a strong tendency to the disease, combined with a nervous temperament; and I venture to affirm that those who will bear in mind my suggestions will find them of the greatest possible benefit in their future practice whenever they meet with cases of this description.—*British Medical Journal*, Nov. 9, 23, and Dec. 7, 1878, pp. 689, 765, 832.

15.—ON IDIOPATHIC, ESSENTIAL, OR PROGRESSIVE PERNICIOUS ANÆMIA.

By Dr. STEPHEN MACKENZIE, Assistant Physician and Lecturer
on Pathology at the London Hospital.

[Dr. Mackenzie gives us some good cases of this kind—a disease too often misunderstood. We give only one of his cases.]

Joseph C., aged forty-seven, a bricklayer, in George Ward. He lived in Norfolk until twenty-one years of age, when he came to London. Has never had ague or syphilis. Though not intemperate, drank beer freely in summer. He says he never had a day's illness until the present, and has always had a fine colour. In October, 1877, he noticed that he felt more tired than usual when he had finished his work. After having his tea he would drop off to sleep each night for an hour or so. He slept well all night, and would get up the next morning feeling quite well. He would have a cup of tea, and shortly afterwards would become nauseated and almost immediately vomit. The nausea and vomiting would last about a quarter of an hour. He did not vomit regularly every morning, but about two or three days in the week. He was not drinking much beer at the time, and when he had drunk more freely he had no morning vomiting. The vomiting continued more or less up to admission. He had

no pain, headache, or other symptoms than those mentioned. About the beginning of December his friends began to say, "What's the matter, you're so white?" In January he had a slight attack of bronchitis. On February 16th, he came to me as an out-patient. I was struck with his great pallor, his feeble soft pulse, and listless manner. I examined him most carefully all over, tested his urine, looked at his eyes with the ophthalmoscope, but could find no cause for the anæmia. The next week he presented himself again. Again I examined him thoroughly, and again failed to find a satisfactory explanation of his anæmic state. He came the third week; his anæmia, always profound, had increased yet more. He was weaker and worse in every way, in spite of the tonic treatment pursued. I was convinced he was suffering from essential anæmia, and persuaded him to come into the hospital. As I was taking him in I did not examine his eyes that afternoon, but in the evening, when in the ward, and his pupils dilated with atropine, numerous hemorrhages were seen in the retinae. On admission, he was a well-developed man of large frame; large but soft muscles; skin moist and exceedingly soft. His face was remarkable for an entire absence of colour; it resembled white wax discoloured by age; it was too white to look like old parchment. The mucous membrane of the lips, mouth, and nares was almost devoid of colour. Conjunctiva of a faint but distinct yellow colour. The same absence of colour observed in the face was marked throughout the whole surface of the body, and was very conspicuous in the hands and at the matrix of the nails. The lungs were normal, except for slight prolongation of the expiratory murmur. The heart's impulse was very feeble, not external to nipple line. There was a harsh but not loud systolic murmur at the apex, heard also over the aorta and middle of sternum; pulse medium-sized and compressible. Abdominal wall thickly lined with fat; hepatic and splenic dulness natural. Temperature at this time normal. Urine 1135 cc. in twenty-four hours, sp. gr. 1020, amber, contained no albumen, 17 grms. urea.

Ophthalmoscopic examination showed great pallor of fundi; arteries and veins very pale, the latter of nearly the same tint as former, but larger. Discs natural. Numerous hemorrhages in each retina, in nerve-fibre layer, scattered about fundus; they had a greater length than breadth, and were generally in the direction of the retinal nerve-fibres. The centre of some of the hemorrhages was of a pale reddish-grey tint, in some places it was white, and there were a few small white patches, of dull aspect, scattered about the fundus.

Numeration and Microscopic Examination of the Blood.—Estimated by Gowers' hæmacytometer, the corpuscular richness

was 18·6 per cent., or 930,000 coloured corpuscles to the cubic millimetre. When a drop of blood was placed on a slide and examined, the corpuscles formed rouleaux, but not long ones. Most of the coloured corpuscles were of natural size, of less deep colour than natural. A few of the coloured corpuscles were of much smaller size than the majority, of deeper colour, with a distinct nucleus, eccentric in position. When irrigated with distilled water, they parted with their hæmoglobin readily, leaving a well-marked nucleus. The number of colourless corpuscles appeared natural, certainly not increased. Much highly refractive granular matter (Schultze's granular masses) was present, and fibrine threads formed rapidly and in great abundance. I dictated the following remark: "The extreme anæmia, without any loss of flesh, and without any sign of organic disease of a nature to account for the anæmia, would point to the diagnosis of essential or pernicious anæmia. This opinion is strengthened by the great deficiency, the small size, and nucleated condition of the coloured blood-corpuscles, and confirmed by the presence of retinal hemorrhages. It is to be observed that the patient has no tenderness over any of his bones." From the date of his admission to the present time his condition may be briefly summarised:—The pallor has increased, the skin assuming a "faded leaf" tint. He has complained of little except extreme weakness. At first he was kept in bed for some weeks; subsequently he was allowed to get up and go into the garden. He found it tired him very much going up and down stairs. He has lately found it an effort to walk about the ward even, and has taken to his bed from sheer weakness. Vomiting, which did not trouble him much soon after admission, has lately returned every day or two. Occasionally he has slight headache. The retinal hemorrhages have come and gone from time to time. He has had attacks of moderate pyrexia without assignable cause. His temperature is usually normal, but every now and then it runs up for a few days to about 100° F. or 101°; a few days ago it reached 102°; but has now subsided to normal. His urine has been frequently examined; it is always free from albumen, and the daily excretion of urea has always been considerably below the normal. Numeration of the blood-corpuscles showed on the 27th of March 18·8 per cent., or 940,000 per cubic millimetre; on the 7th of May 19·6, or 980,000 per cubic millimetre; whilst yesterday (June 3rd) there were only 13 per cent., or 650,000 per cubic millimetre. Many of the corpuscles have been observed, on each occasion, to be small and nucleated. You will see him as he lies in bed, bloodless to a degree, listless and apathetic, complaining of nothing unless specifically questioned. [After relating other two cases, Dr. Mackenzie remarks]:—

These cases illustrate the general features of the disease I wish to discuss. Let me briefly gather up the important points I wish most to impress upon you. The disease generally commences insidiously; the patient becomes paler and weaker, but it is remarkable that he does not lose flesh. In most cases the pallor becomes extreme. The skin is of velvety softness, and of a pale yellow, wax-like or ivory-like appearance, sometimes most resembling the skin of the cadaver. No abnormal physical signs are found in the viscera, beyond loud anæmic bruits heard over the heart and large vessels, and slight prolongation of expiration at the apices of the lungs. Examination of the blood reveals deficiency and alteration in the form and colour of the coloured corpuscles; no increase in the colourless corpuscles. The patient meanwhile complains of intense prostration, appears indolent and incapable of exertion, and is often sleepy, so that he drops off to sleep whenever he sits down. He suffers from palpitation and breathlessness on the least exertion. Vomiting, especially after food, is generally present at some period of the disease, and often becomes excessively distressing in the late stage. The patient generally complains of vertigo, and sometimes has hallucinations or slight delirium. Febrile attacks occur from time to time without definite causation. Finally, there arrives a period of hemorrhages. Ophthalmoscopic examination shows, in most cases, hemorrhages into the retina, and, rarely, swelling of the optic nerve. The retinal hemorrhages are usually a late phenomenon of the disease. Epistaxis is common; bleeding takes place from the gums, and very often, in women, from the vagina; rarely there are cutaneous hemorrhages. Hemorrhages also occur into internal organs, as the brain, and into the mucous membranes and muscles. Cases occur, however, without any such hemorrhages. Death is usually preceded by œdema; a little swelling about the feet, hands, or face, and into the serous cavities. This may suggest Bright's disease, but the urine is found free from albumen and casts. Post-mortem examination shows extreme pallor of all organs, with fatty degeneration of some—the heart, liver, and kidneys especially; submucous and subserous hemorrhages in some cases. Usually the spleen and lymphatic glands are natural, though sometimes the former is enlarged. The marrow of the bones shows remarkable changes in some cases. It appears to the naked eye red instead of yellow, and the microscope reveals an excess of lymph-cells and nucleated blood-corpuscles. The blood is pale-fawn-coloured, fluid, and with little tendency to clot. The microscope has also shown in some cases, especially in nervous structures, aneurismal dilatation of capillaries and arterioles, with degeneration of the walls of the vessels, perivascular extravasation, and cellular infiltration.

These aggregate symptoms observed during life, coupled with the appearances found after death, stamp the disease with an individuality of its own, rendering it capable of recognition to those familiar with its characteristics. Neither the disease nor its recognition is new. The disease has been observed and described for a considerable time, but, owing perhaps to the indefiniteness of the malady, perhaps to few having read the original description of it, it has not until quite recently gained general professional attention, but has remained confined to the knowledge of comparatively few physicians. Its description finds no place in most of the modern text-books of medicine. Thanks to the teaching of Dr. Sutton, and the writings of Dr. Wilks calling attention to Addison's description, I have been familiar with the disease since a student, and have seen characteristic cases from time to time. To Dr. Addison is undoubtedly due the credit of first separating the disease as a clinical entity. He described the disease in his lectures delivered in 1843, which were published in 1853. Dr. Wilks recorded cases in Guy's Hospital Reports for 1847-8 and 1857. Lepine, in a careful historical review of the literature of the disease, ably abstracted and supplemented by Dr. Pye-Smith, shows that whilst Andral, Piorry, Wunderlich, Canstatt, Schönlein, and Lebert had described cases of the disease, chiefly as chlorosis or anæmia following parturition, Addison was the first to describe its clinical features, progressive course, fatal termination, and characteristic post-mortem appearances. Gusserow, in 1871, described six cases, and Biermer, in 1872, fifteen cases of the disease, observed at Zurich, and at first believed by them to be peculiar to that locality. Biermer, ignorant of the existing literature on the subject, described it as a new disease under the title of "progressive pernicious anæmia." And though no candid critic could allow of Biermer's claim to its discovery, there can be no doubt his study of the disease has advanced our knowledge concerning it, and that his memoir on the subject has awakened the general attention of the profession abroad and in this country. Addison's description remains the best on the subject, and since this is the case, and the fact is important in medical history, I will immediately read you what he says in connection with it. On Immerman reporting two cases confirming, as he thought, the discovery of the disease by Biermer, Dr. Wilks, who has so often brought before the profession the overlooked writings of his illustrious colleagues Addison, Bright, and Hodgkin, promptly drew attention to the fact that the disease had long been recognised in this country, and described by Addison and himself. Since then numerous cases have been reported by observers at home and abroad, among whom may be named Pye-Smith, Bradbury, Byrom-Bramwell, Broadbent,

Gardner and Osler of Montreal, Pepper, Lepine, Immerman, Rosenstein, Litten, Quincke, Eichorst, and others too numerous to mention, but to whose writings reference will be found in Dr. Pye-Smith's *résumé* in the London Medical Record. Professor Quincke's lecture contains a valuable description of the disease. Immerman has, as an appendix to his article on Anæmia in Ziemmsen's Cyclopædia, a chapter on Progressive Pernicious Anæmia (Biermer), but he makes no allusion to Addison's writings.

Addison wrote:—"For a long period I had from time to time met with a very remarkable form of general anæmia, occurring without any discoverable cause whatever—cases in which there was no previous loss of blood, no exhausting diarrhoea, no chlorosis, no purpura, no renal, splenic, miasmatic, glandular, strumous, or malignant disease. Accordingly, in speaking of this form in a clinical lecture, I, perhaps with little propriety, applied to it the term "idiopathic," to distinguish it from cases in which there existed more or less evidence of some of the usual causes or concomitants of the anæmic state. The disease presented in every instance the same general character, pursued a similar course, and, with scarcely a single exception, was followed after a variable period by the same fatal result. It occurs in both sexes, generally, but not exclusively, beyond the middle period of life, and, so far as I at present know, chiefly in persons of a large and somewhat bulky frame, and with a strongly marked tendency to the formation of fat. It makes its appearance in so slow and insidious a manner that the patient can hardly fix a date to his earliest feeling of that languor which is slowly to become so extreme. The countenance gets pale, the whites of the eyes become pearly, the general frame flabby rather than wasted; the pulse perhaps large, but remarkably soft and compressible, and occasionally with a slight jerk, especially under the slightest excitement; there is an increasing indisposition to exertion, with an uncomfortable feeling of faintness or breathlessness on attempting it; the heart is readily made to palpitate; the whole surface of the body presents a blanched, smooth, and waxy appearance; the lips, gums, and tongue seem bloodless; the flabbiness of the solids increases, the appetite fails, extreme languor and faintness supervene, breathlessness and palpitation being produced by the most trifling exertion or emotion; some slight œdema is probably perceived about the ankles. The debility becomes extreme; the patient can no longer rise from his bed; his mind occasionally wanders; he falls into a prostrate and half-torpid state, and at length expires. Nevertheless, to the very last, and after sickness of perhaps several months duration, the bulkiness of the general frame and the obesity

often present a most striking contrast to the failure and exhaustion observable in every other respect. On examining the bodies of such patients after death, I have failed to discover any organic lesion that could properly or reasonably be assigned as an adequate cause of such serious consequences, &c."

The little we have learned since Addison described the disease has been gained chiefly by means of the microscope, and in a slight measure by the ophthalmoscope. The clinical features of the disease are now generally known and agreed upon, and further elucidation of its essential pathology must be relegated to the histologist.

Let us now inquire a little more closely into some of the more important and interesting features of the disease, and first into its

Causes.—Mental shock or emotion has been shown, in several cases, to be the starting-point of the disease. Thus, in a case related by Sir H. Marsh, a young lady accidentally poisoned her father by giving him the wrong medicine. She was so overwhelmed by grief that she took to her bed, became anæmic, and died. In another instance a young man saw a child run over in the street; he was very much shocked at the occurrence, and never felt well from that date; he became progressively anæmic and weak until death, when nothing abnormal was found in his body except a bloodless condition of the viscera. In another case a young lad died of extreme anæmia in Guy's Hospital, under Sir William Gull. He had never been well since he had been attacked by a sheep in a field. Grief and brooding over the loss of relatives is recorded in several cases. Parturient women, who have not lost much blood, seem especially prone to the disease. Our German *confrères* appear to think the disease more common in the badly-cared for and impoverished, but such is by no means our experience in England. The well-to-do suffer quite equally with the poor, and experience confirms Addison's observation that it is apt to occur in persons predisposed to be fat. It may be said to occur in both sexes with about equal frequency. The majority of recorded cases in this country have been males, in Germany females; but the difference may lie in the sources of the patients, and the specialties of observers. It is more common at and after middle life. Dr. Fenwick has shown the existence of atrophy of the stomach in cases of idiopathic anæmia, but it does not appear quite clear as to whether the gastric atrophy stands in the position of cause or effect. If the former, its rôle is obvious. By arresting nutrition at its very portal the elements for healthy cytogenesis are wanting. Dr. Gowers has suggested that it may be an unusual or irregular manifestation of malarial poisoning. In tropical climates

extreme anæmia is very common, but not necessarily malarial in origin. Dr. Sullivan says—"The anæmic condition is a fit soil for the development of malaria in all its varied manifestations." It must be remembered, however, that malarial acclimatisation does not afford complete protection against active malarial manifestations. In the majority of cases no assignable cause can be discovered for the occurrence of the disease, its peculiar progressive character, and its obstinate rebellion against remedial measures.

Course.—The progress of the disease is almost always steadily downwards. From its insidious onset it is often only recognised in a very late stage. It generally runs its course in about six months, or in from six to twelve months. Patients may live with the disease even for some years; and though it is usually progressive in its course, it is sometimes temporarily arrested, and in rare cases even recovered from. All the symptoms increase in severity very gradually, and the progressive weakness is sometimes so slow and gradual that the patient appears almost lifeless for some little time before death. Sudden exertion may cause severe collapse or syncope, and hasten the end, and in this way a patient in apparently a stationary condition may run down very rapidly, especially on removal from his home to a hospital, or *vice versa*.

Let us now consider some of the special symptoms under the following heads.

(a) *Appearance.*—This is quite peculiar. The patient looks as bloodless as a person suffering from profuse hemorrhage. The mucous membranes and skin lose nearly all trace of the colour imparted to them by healthy blood. The lips, gums, tongue, mucous membrane of nares, vagina, &c., become blanched. The skin varies in tint from marble whiteness to a pale-lemon colour. In some cases it looks like a wax cast discoloured by age, or like discoloured ivory. Sometimes it has the "faded leaf" appearance, just such as is seen in cancerous cachexia. The loss of colour involves the whole integument, but is necessarily most conspicuous in the most vascular parts. Thus it is especially noticed in the face and in the hands; on holding the latter in juxtaposition with a healthy person's hands there is seen to be an entire absence of the natural pinkness of the flesh and of the matrix of the nails. In the late stage the skin may assume a deeper shade, or yellowish-brown tint. The conjunctiva is usually colourless and pearly, the inner surfaces of the lids being of milky whiteness; but not unfrequently it has a distinct though faint yellow tint, as in C. and P. This, I believe, is due to the subconjunctival fat acquiring a yellow colour, for it is noticeable that, on post-mortem examination, the fat of the body is often of an unusually bright yellow or

canary colour. It is, however, sometimes due to staining of the tissues with the pigment of disintegrated blood-corpuscles. It is highly important to remember that the conjunctiva may have this yellow tint, as it sometimes leads observers astray in thinking they have to do with a case of jaundice. No bile pigment is found in the urine to indicate that there is bile circulating in the blood.

(b) *Digestive troubles*.—These are usually present from the onset of the disease. Vomiting is a very frequent symptom, especially in the morning and after food. It becomes especially troublesome, usually towards the close of the disease. Patients complain much of pain and fulness after food; that is, they have the symptoms of atonic dyspepsia. Dr. Fenwick's researches afford an explanation of this. Often there is troublesome and recurring diarrhoea, which helps to exhaust the patient; but sometimes the opposite condition obtains, and the bowels are torpid. We come now to very important considerations.

(c) *Cardio-vascular symptoms*.—The heart nearly always gives indications of feeble action. The patient is usually troubled by palpitation, often when at rest, or readily excited by the least movement or emotion. On examination, the heart's action is found rapid and variable, sometimes intermittent. Murmurs are usually heard, always systolic in time; sometimes they are soft and blowing, more usually loud and harsh, but still maintaining a blowing character; usually loudest at the base, they are generally heard over a wide area; sometimes they are loudest at the apex of the heart, conducted into the axilla, and heard at the angle of the left scapula. It is remarkable how loud and harsh these bruits sometimes are. This has been explained by Quincke as due to the retraction of the anterior border of the lung, which occurs in the anæmic, thus exposing a larger part of the heart, and allowing of better conduction. Occasionally the cavities of the heart are enlarged, and then there is a probability of valvular imperfection, though not necessarily of valvular disease. As regards the production of the bruits heard, these have been explained by Immerman and Lepine in the following way. On post-mortem examination the heart, as I have already told you, is almost invariably found affected with fatty degeneration. It manifests itself by pale yellow zigzag mottlings beneath the endocardium (tabby-cat striation), caused by fatty degeneration of the muscular fibres, irregularly distributed. These markings of fatty degeneration are found with especial frequency on the papillary muscles. Muscle so much diseased must necessarily fail in function, and it is thought that during the systole, the degenerated papillary muscles failing to contract with sufficient force,

the cuspid valves are forced into the auricles, and the blood regurgitates from the ventricles into the auricles. It may be, however, that the bruits heard are due wholly or in part to lessened viscosity of the blood, for we hear equally loud murmurs in cases of chlorosis and artificial anæmia, where we cannot believe, from the rapidity of their production and their disappearance, that they have owed their origin to regurgitation from incompetency produced by fatty degenerated papillary muscles. A loud continuous murmur (*bruit de diable*) is almost invariably heard over the jugular veins at the root of the neck. This is generally, but incorrectly, believed to be an indication of anæmia. As my colleague, Dr. Herbert Davies, has shown by careful and numerous investigations, often overlooked, "it is not an anæmic or chlorotic murmur, though uniformly present in those conditions of the system which are marked by an impoverished condition of the blood, inasmuch as it has been observed in a multitude of instances to co-exist with the ruddiest complexion and the most perfect health." The arteries are empty, the pulse small, soft, and jerky; sometimes there is atheroma of the large arteries.

The blood presents remarkable alterations. The coloured corpuscles, hæmoglobin, and albumen, are deficient; the watery part of the blood increased; the colourless corpuscles, as a rule, unaltered, either in numbers or appearance, but, owing to the great deficiency of the coloured blood-corpuscles, they are *relatively*, though not actually, increased. The coloured blood-corpuscles are not only diminished in number, but they present conspicuous alterations in their size, colour, and appearance. *Diminution in the number of blood-corpuscles* has long been noticed in the anæmic state, but the observations on this point have lacked precision for want of readily applicable means of numeration. Several instruments more or less fulfilling the conditions of exactness and simplicity have been recently devised, the "compte globule" of Mallassez and the instrument of MM. Hayem and Nacet being the best known. Dr. Gowers has modified and greatly improved Hayem and Nacet's instrument, and his "hæmacytometer" made by Hawksley is a really simple instrument, which can be used at the bedside in ordinary practice. In Dr. Gowers's "hæmacytometer" the floor of the cell which holds the diluted blood is ruled in tenth of a millimetre squares. The number of corpuscles in ten squares is counted, and this, multiplied by 10,000, gives the number of corpuscles in a cubic millimetre of blood, the usual way of expressing the corpuscular richness. Vierordt and Welcher have stated that the average of healthy blood is 5,000,000 per cubic millimetre. In cases of moderate anæmia the number of coloured corpuscles sinks to four to three millions per cubic millimetre, in marked cases to

two to one million. The number may sink as low as half a million in the cubic millimetre, but this seems the limit at which life can be sustained. In the case of C. the number has sunk to nearly this degree (650,000), and this presages an early fatal issue. The corpuscular richness, as I have said, is usually stated as the number in a cubic millimetre; but Dr. Gowers, by employing a dilution of 1 in 200, instead of 1 in 250, and using a scale of squares a tenth of a millimetre, is able, by counting the number of corpuscles in two squares (the mean of ten or ten or twenty) to express the centesimal richness of the blood. For, "taking five millions as the average per cubic millimetre for healthy blood, the average number in two squares of the cell is 100. These two squares contain .00002 cubic millimetre of blood, and it is proposed to take this quantity as the hæmic unit. The number per hæmic unit—*i.e.*, in two squares—thus expresses the percentage proportion of the corpuscles to that of health, or, made into a two-place decimal, the proportion which the corpuscular richness of the blood examined bears to healthy blood taken as unity." This centesimal expression of the corpuscular richness is a great simplification and improvement. Apart from the saving of calculation, it enables us much the more readily to bear the numbers in our memory. You must not suppose that diminution of coloured corpuscles is a peculiarity of idiopathic anæmia. It is found in most acute and in all chronic wasting diseases. Thus Sorensen has shown that in pneumonia (eleven cases) the average was 4.43 millions per cubic millimetre; in rheumatic fever (nine cases) 4.16; in typhoid fever (eleven cases) 4.82; in phthisis (eleven cases) 4.35; in cancer (six cases) 3.66 millions. Mr. Major Greenwood has counted for me the blood-corpuscles in several cases now in the wards, with the following results:—A case of catarrhal jaundice, with great apparent anæmia, 79.7 per cent., or 3.985 millions; very chronic catarrhal jaundice 47.2 per cent., or 2.36 millions; chronic parenchymatous nephritis 62.2 per cent., or 3.11 millions; chronic interstitial nephritis, looking very anæmic, 75 per cent., or 3.75 millions; heart disease, looking very anæmic, 72.2 per cent., or 3.61 millions; abdominal carcinoma (very cachectic) 81 per cent., or 4.05 millions; phthisis (extreme emaciation) 47.2 per cent., or 2.36 millions; cancer of pancreas, with pyloric obstruction (emaciation very extreme), 51.2 per cent., or 2.56 millions per cubic millimetre. In this last patient's case, his weight, at the time of observation, was compared with that when he was in health; and it seemed that the hæmophthisis (wasting of blood) was exactly proportionate to the wasting of tissues shown by his decrease in weight. In none of these cases, it will be observed, did the deficiency of corpuscles approach that of cases of idiopathic anæmia—the 23.6 per cent. of P., or

the 13 per cent. or 0.650 million per cubic millimetre of C. Dr. Gowers has, however, recorded a case of chlorosis where the corpuscular richness was as low as 1,300,000 per cubic millimetre, or 26 per cent., and which improved whilst taking iron, so that the corpuscles reached 70 per cent. The patient had a striking diminution of corpuscles at each menstrual period.

Pathology.—This still remains shrouded in obscurity. It is as yet undecided whether the deficiency of coloured blood-corpuscles, which evidently underlies the question of the essential nature of the disease, is due to defective formation of the coloured blood-corpuscles (anhæmatisis), or to increased destruction of the corpuscles (hæmophthisis) of the cytogenic (or blood-elaborating) organs. The marrow of the bones has given the most marked evidence of implication, the spleen having, in a few cases, shown alterations. The liver has afforded somewhat indefinite indications of blood destruction. The lymphatic and mesenteric glands, and lymphatic structures of the intestine and elsewhere, have not been proved to be involved. As regards the bone-medulla, the minute anatomy and physiology of which have only recently been studied with care, it remains uncertain whether the changes which have been found in idiopathic anæmia are peculiar to that morbid state; and, if so, whether they stand in the relation of cause or consequence. The former view has been adopted by Cohnheim, Osler, and some others, who have regarded the nucleated and dwarf corpuscles found in the blood as due to arrest of their development—"deformed, unfinished, sent before their time"—owing to failure of the cytogenic function of the marrow of the bones. The opposite opinion is held by Neumann (who has devoted so much study to the marrow), Litten, and Orth. They assert that the changes in the marrow are not peculiar to any one form of anæmia, but are brought about by the anæmic state, however produced. Neumann, moreover, points out that the large collection of blood-discs found at the diaphyses of the hollow bones, where only fat-containing marrow should exist, cannot be taken to indicate arrested production of corpuscles, but must be regarded rather as a new source for them. Neumann regards the changes in the marrow as a compensatory effort of the organism to supply the deficiency created by great destruction of the coloured blood-corpuscles. There can be no doubt of the close alliance of the disease with the other cytogenic diseases—anæmia lymphatica (pseudo-leukæmia), leukæmia splenica and lymphatica, and myelogenous leukæmia. In these various states we see the blood profoundly altered, either by an increased production of colourless corpuscles, actual or relative, or by an arrest of the development of the coloured cor-

puscles from the latter, these conditions depending upon, or being associated with, changes in the blood-forming organs, the spleen, the lymphatic glands, the marrow of the bones. With myelogenous leukæmia, idiopathic anæmia has the closest alliance. In each the disease appears confined to the marrow of the bones; in the former with increased development of colourless corpuscles (or total arrest of evolution of coloured from colourless corpuscles); in the latter, with no increase of colourless corpuscles, but with imperfect formation of coloured corpuscles (partial arrest of evolution of coloured from colourless corpuscles.) Cases have been recorded which began as anæmia and terminated in leukæmia, the lesion being found in the marrow of the bones. I repeat, however, it still remains uncertain how far these changes are the cause, how far the effect of the disease. The fatty changes in the different organs are the result, and not the cause of the anæmia. Perl and others have produced fatty degeneration of the heart in dogs by repeated bleedings. It is probable, as several writers have observed, that in idiopathic anæmia we have to do not with one morbid state dependent upon a single anatomical change. As Immerman says, we must regard the term idiopathic, or progressive pernicious anæmia as "a sort of provisional shelter for a multitude of cases, possibly of various origin." It will be the duty of each subsequent clinical and pathological investigator to reduce the number of cases embraced under this term, by relegating them to classes according to their etiological relations or the primary organ or tissue at fault. This is a convenient place to discuss the name which should be attached to the malady. The name Addison gave, with some diffidence, to the disease is not without objection. We do not believe the disease arises spontaneously, though we may remain in ignorance as to its origination. The word "idiopathic," almost banished from our nosology, excites a mental attitude of antagonism, which, however, perhaps, has the advantage of inciting investigations as to the causes of the disease. The term "essential" expresses well the clinical and anatomical features of the disease. Biermer's title, "progressive pernicious anæmia," embraces the general clinical characters of the disease; but the term "progressive" is better omitted, for the disease is sometimes, though rarely, arrested. In the interest, however, of medical science and history, I think it is better to retain the name "idiopathic anæmia," first given to it by Addison, and thus refer the inquirer back to the original and masterly description of the disease by the great English physician.

The *prognosis* is always very grave. Sooner or later, in all but a very few cases, the blood poverty reaches a stage at

which life cannot be supported. In this respect Biermer's title of "progressive pernicious anæmia" is well founded. The prognosis is most serious in the young of the male sex, and in pregnant women, who generally die rapidly, after aborting. Anæmic fever and hemorrhages are unfavourable signs, indicating an advanced stage of the disease; whilst dropsy, with rare exceptions, portends a rapid termination of the case. Though usually progressive, the disease may be arrested for a short period, or even remain stationary for some years. I have known a middle-aged woman in this hospital who had characteristic symptoms of the disease for five or six years, and who left the hospital slightly improved by treatment. I have already mentioned, in speaking of the course, that the disease usually terminates in from six to twelve months.

We come now, lastly, to consider the *treatment*. With the confession of our ignorance of its pathology, and from the evil name the disease has acquired, you will probably have inferred that treatment has not hitherto been at all successful. Iron, which seems so strongly indicated, generally fails to arrest the disease, or to produce its usual effects in increasing the number, size, and colour of the blood-discs. Unless gastric symptoms contra-indicate its employment, it should always be tried, alone or in combination. Bland's or Vallet's pills are powerful hæmatics. In chronic cases the natural chalybeate waters suggest themselves as the most proper vehicle for the iron. Phosphorus and nitric and nitro-hydrochloric acids have been found of no avail. Arsenic has proved in Dr. Byrom Bramwell's hands of more signal service than other remedies. Arsenic has long had a reputation as a blood tonic. From its marked effect in producing dispersion of lymphatic enlargements (as occasionally in Hodgkin's disease) it would appear to have some special influence upon all the cytogenic organs, though possibly greater upon the lymphatic than the myelogenous. It may be used in combination with iron, Fowler's solution and the tartrate agreeing well together; or arsenious acid may be given with sulphate of iron in pill. Transfusion of human and of lamb's blood has been tried in several cases, but not with encouraging results. The most important point in the treatment, however, is the careful attention to physical and psychological hygiene. The patient should be kept warm, and in bed. We must remember that his oxygen-carriers are fewer and feebler than in health, and we must therefore save him from any unnecessary demands upon their functions in the processes of respiration and circulation. By keeping the patient in bed we minimise their work. Remembering that gastric atrophy is frequently associated with, if not an essential factor of, the disease, explaining the repugnance to food, the gastralgia, and

the vomiting, so generally present, only the most assimilable food should be given, in small quantities at a time. We have seen that the disease sometimes owes its origin to mental shock and depressing emotion. The cerebral anæmia itself tends to keep up or produce a restless, irritable, and depressed state of mind. It is of the highest importance to combat this by every means in our power. The horizontal position is on this account, also, to be maintained. The patient should be kept in a light, bright, and cheerful apartment, where, if possible, the sun's rays enter some part of the day. We should try by suitable companionship, and by encouraging and helpful words to keep up a happy train of thought, and a hopeful attitude of mind.—*Lancet*, Dec. 7 and 14, 1878, pp. 797, 833.

16.—ON BLOODLETTING.

By Professor T. WHARTON JONES.

At the present day medical practitioners seldom order abstraction of blood by venesection in cases of inflammation, and have even misgivings as to the propriety of applying leeches, the current doctrine being that it is not necessary for the cure, and that it involves a risk of impairing the strength needed to bear up against the disease. Formerly quite the contrary opinion prevailed, so that it would have been considered bad practice to abstain from the use of the lancet in any inflammatory complaint whatever. Bloodletting was, therefore, very commonly prescribed, but the extent to which it was sometimes pushed, especially by repetitions of the venesection, it must be admitted, was anything but reasonable, and, no doubt, reform was imperatively called for. Instead of simple reform of indiscriminate practice, however, complete reaction in the opposite direction has taken place, as is so common when extreme views on any subject come to be called in question. And now, as I before observed, doctors abstain from venesection, or even leeching, and that to the most moderate amount, in almost any case of inflammation, however severe.

Under these circumstances, in face of such a difference between past and present opinion, it may fairly be asked—and the question I consider one to which it is of the highest importance to obtain an answer, founded on the dictates of science and experience,—under these circumstances, I say, it may fairly be asked whether by abstention from bloodletting altogether, as is the present fashion, inflammations of important organs are not often allowed to run a prolonged and disastrous course, which might be prevented by a timely abstraction of blood in such quantity that the loss of it could be in no wise injurious to the patient?

This question I will venture to answer as regards an important inflammation of the eye—namely, iritis. And first, I would observe that, in my experience, blood may be safely drawn, in any case in which we would think of letting blood at all, to the amount of ten to twenty ounces, while in many cases a repetition of the venesection to the same quantity need not be feared. You will have opportunities of observing in cases of iritis under my treatment here, that when bleeding has been had recourse to at the early stage of the disease, the pain around or over the orbit, or in the temple, ceases, or is soon greatly mitigated, the feeling of depression of the strength—for it is only a feeling, not real weakness—is relieved, the vascular congestion of the eye subsides, the pupil yields more completely to atropia, while mercury operating with more effect, less of it requires to be given. Convalescence is thus established within two or three weeks, sometimes even one week, with strength not only unimpaired but actually improved. *Apropos* of this, let me relate a case. My friend the late Mr. Graham, formerly Professor of Chemistry in this College, and latterly Master of the Mint, was seized with rheumatic iritis, for which I attended him in conjunction with the late Sir Robert Carswell, then also a professor in this College. Though Mr. Graham was not a strong man, I bled him, and, with the concurrence of Sir Robert Carswell, repeated the venesection two or three days after; calomel with opium internally, and a belladonna application externally, were at the same time prescribed. Within a fortnight our patient was convalescent and back again to his lectures at the College, with strength not only unimpaired but actually improved. Though once before this he suffered from an iritis, for which he was successfully treated by the late Dr. Mackenzie, of Glasgow, he never had another attack. But mark you this to show how much Mr. Graham appreciated the benefit he had derived from the bloodletting: some years afterwards, finding himself rather out of health generally, he called on me one day and asked my opinion as to whether a bloodletting on this occasion was as likely to do him as much good as before. I was happy, however, to assure him that there was not now any necessity for the abstraction of blood; and to set his mind completely at rest I took him to the late Sir James Clark, who concurred in my opinion.

Without bloodletting, convalescence will not, in similar cases, take place in double the time, or even longer, and that only imperfectly; for, as a result of such a prolongation of the inflammatory action, serious organic damage of the eye is liable to accrue in the shape of synechia posterior, and implication perhaps of the ciliary body, choroid, and retina; whilst by

the prolonged treatment the strength of the patient is pulled down, and a great tendency to relapse from slight causes remains.

In such a damaged condition of the eye, iridectomy often does good as a "no better do," but odd it is that the surgeon who would not hesitate to put the patient under the influence of chloroform or ether, and forthwith perform this operation, would, perhaps, shrink from ordering the more simple one of venesection—nay, even from the application of a few leeches—at the beginning of the iritis, by the beneficial operation of which the damaged state of the eye, requiring iridectomy, might have been prevented.

I have thus instanced the advantage of bloodletting in iritis, because we can so well observe its effects from day to day, and because I can speak from experience on the subject, elucidated by scientific observation as to the pathology of the inflammatory process generally. The *process*, mark me; for the state you have to contend with at the beginning of a pure, uncomplicated, acute inflammation, is very different from that which has supervened in the order of things at a later stage. First in the series of changes which take place in the inflammatory process, of which a part of the body accessible to observation is the seat, there is the visible rubor or redness, and the invisible nervous and muscular action which led to the vascular congestion on which the redness depends; while in the later stages there are the visible manifestations of the exudation of lymph, the tumour or swelling, &c., which have supervened on the congestion by the invisible work of liquid diffusion and cell-multiplication. Abstraction of blood in the beginning of an inflammation relieves the congestion, and thereby prevents or limits exudation. By abstraction of blood in the later stage, after exudation has taken place, congestion will even then be relieved, though less quickly, and in proportion as re-establishment of the circulation in the inflamed part takes place, the exuded matters tend to be absorbed, and so the inflammatory process slowly comes to an end.

The doctrine that the inflammatory process consists merely in "proliferation" virtually ignores the vascular congestion and the symptoms depending on it, such as *rubor cum calore et dolore*. The doctrine thus refutes itself, by omitting cognisance of the condition on which depends the supply of material from the blood for the maintenance of the increased activity of cell-multiplication constituting "proliferation." By disallowing vascular congestion as an all-important stage in the inflammatory process, the doctrine of proliferation supplies a plausible argument against the necessity of bloodletting, easily admitting, however, of being disproved by clinical observation and experience.

A peculiar incompressibility of the artery in feeling the pulse, hot skin, and white tongue are indications of the propriety of bleeding in general. They are so in iritis, in particular, and, I may add, catarrho-rheumatic ophthalmia also, in which the orbital or temporal pain and apparent depression of the strength are often much greater than in iritis, whilst danger to the integrity of the eye is as great, though in a different way—viz., by rapid ulceration to penetration of the cornea with all its disastrous consequences, such as prolapsus iridis, &c.

Irrespective of the general indications I have mentioned, the local subjective and objective symptoms in each case of iritis or catarrho-rheumatic ophthalmia will, often of themselves, furnish sufficient indications for the abstraction of blood. Circumorbital or temporal pain, for example, is a good indication, for when an acute ophthalmia in an ordinarily strong person is attended by that subjective symptom, venesection, followed by calomel and opium, quickly gives relief. And the same may be said of the ear-ache in acute inflammation of the membrana tympani. The absence of such pain, however, would be no counter-indication in the face of well-marked objective symptoms.

In severe inflammations of important organs the system tolerates the loss of blood in greater quantity than when there is no inflammation present, or when the organ inflamed is of less vital importance in the economy. In illustration of this fact the late Dr. Marshall Hall, in his work "On the Morbid and Curative Effects of Loss of Blood," pointed out from his experience that if several patients affected with dissimilar complaints be bled, while sitting in the erect posture, it is found that one patient, perhaps, will faint on losing only a few ounces of blood, while another will stand the loss of ten times the quantity before syncope takes place.

The complaints in which the greatest quantity of blood may thus be drawn without the speedy induction of syncope are: inflammatory congestion of the brain, inflammation of serous membranes, such as pleuritis or peritonitis, and inflammation of the parenchymatous substance of organs, such as pneumonia, to which may be added, as before mentioned, iritis and catarrho-rheumatic ophthalmia. Now take notice that these are the very complaints in which abstraction of blood is of most benefit, and, therefore, most called for. Dr. Marshall Hall accordingly laid it down as an axiom that in the operation of venesection the quantity of blood lost before fainting comes on, whilst the patient is in the erect posture, is *never more than is requisite for the cure of the inflammation, and never so great as to prove hurtful to the patient.*

In regard to the propriety of repeating venesection, the late

Mr. Wardrop remarked that of a number of persons bled for inflammatory diseases, those who have lost the *largest quantities of blood by the fewest repetitions* of the operation have made the most rapid recoveries.

One moderate bleeding at first—say, to sixteen ounces—is usually sufficient for the speedy cure of an iritis or catarrho-rheumatic ophthalmia: but in some cases it may be necessary to repeat the venesection. The continuance of great pain in and around the eye, much vascular congestion, and the formation of a buffy coat on the blood previously let, will generally, taken in conjunction with other circumstances, indicate the propriety of a repetition of the venesection. If the first bleeding was well borne, a second may safely enough be had recourse to; but it is to be remembered that fainting comes on from the loss of smaller and smaller quantities of blood in the subsequent operations; showing that repetitions of the venesection are less and less necessary.

When it is not considered proper to repeat venesection, the application of leeches to the affected organ may still be necessary and prove advantageous. Leeches alone, applied near the corners of the eye, without any preceding venesection, you will often find sufficient for the speedy cure of moderate degrees of iritis and catarrho-rheumatic ophthalmia. It being always, of course, understood that the appropriate medicinal treatment is employed.

In acute inflammation of the membrana tympani, attended by ear-ache in children, a leech or two behind the ear and a few doses of gray powder give speedy relief, and, by arresting the inflammation, prevent damage to the structure and function of the ear. In grown-up persons, if the pain in the ear be very great, venesection may be required, followed up with blue-pill or calomel, colchicum, and Dover's powder.—*Lancet*, November 2, 1878, *p.* 613.

17.—GARGET AND DIPHTHERIA.

By Dr. F. P. ATKINSON, Kingston-on-Thames.

From the description given by Mr. Smee, at the Pathological Society, of that form of cow-disease called “garget,” it would appear to be of an ordinary inflammatory character, arising sometimes from cold, sometimes from blows, sometimes from retained secretion, and sometimes idiopathically. When it results from cold, there would seem occasionally to be enlargement of the glands of the neck, and here and there paralysis of the legs, mostly the hinder ones. Were it a disease of a septic character, it would certainly be conveyed to almost every cow in the drove; and, were it of a distinctly diphtheritic nature,

we should expect to find patches of membrane in the throat and on the udders. If diphtheria were capable of being conveyed to man by the milk of a cow which is simply suffering from a disturbed state of the system, then we should be compelled to alter our present belief, that like produces like, and accept the theory of heterogenesis, which, I consider, as yet has no certain foundation whereon to rest.

During the past year, we have had a slight outbreak of diphtheria at Surbiton, caused, I believe, by the mixing of sewage-contaminated water (which had been used for washing the cans) with the milk. It was limited entirely to those who had been supplied with milk from one particular dairy, and ceased almost immediately upon a change being made in the water. If milk, after the process of milking, be capable of contamination, and of producing, as has been proved, scarlatina and enteric fever, it can equally well, I should say, produce diphtheria and cholera. In order to be able to assign the cause of any particular septic disease to the cow, I cannot help thinking it is necessary for the animal to have the particular train of symptoms peculiar to that disease.—*British Medical Journal*, Jan. 25, 1879, p. 113.

18.—GARGET.

By Dr. W. N. THURSFIELD, S.Sc.Camb., Shrewsbury.

Without at all entering into the merits of the theory of a connection between diphtheria in the human subject and some one of the several affections of the udder in the cow known as "garget," I have thought that, at the present time, the following statement of facts might be useful, both as a precedent in dealing with cases of "garget" and as indicating what in one case at least proved a ready chemical test for evidence of that disease in milk.

Eighteen months ago, I received information that, amongst the cows at a dairy-farm, supplying milk for sale to dealers in a town for which I am medical officer of health, was a cow suffering from garget. A veterinary inspector was at once despatched thither, and found this to be the case; the fact, indeed, was not denied by the owner of the cows. All the milk ascertained to have been sent from the farm in question and remaining unsold, was in consequence seized by the inspector, and I gave a certificate that it was unfit for human food, on which it was destroyed by a magistrate's order under Section 116 of the Public Health Act. The sanitary authority, on my report, subsequently gave directions for proceedings for a penalty to be instituted in the case. In the absence of evidence of disease following the consumption of that portion of the milk which

had been sold, it was necessary for me to state in evidence on what grounds I considered the milk to be unwholesome. Although microscopic examination gave undoubted evidence of pus-corpuscles, it was difficult thus to demonstrate small quantities of blood in the milk. The guaiacum test for blood, however, gave a well marked and unmistakable reaction with the garget milk, but not with ordinary milk. To a small quantity of the garget milk on a watch-glass were added a few drops of tincture of guaiacum, and then of solution of peroxide of hydrogen (in the shape of a few drops of sanitas fluid); a beautiful and persistent blue tint was at once developed.

On my evidence that the milk contained pus and blood, a conviction ensued, and a heavy penalty was inflicted.—*British Medical Journal*, Jan. 25, 1879, p. 113.

DISEASES OF THE NERVOUS SYSTEM.

19.—ON THE DIAGNOSIS OF EPILEPSY.

By Dr. J. HUGHLINGS JACKSON,, F.R.S., Physician to the London Hospital, and to the National Hospital for the Epileptic and Paralysed.

The physical process in a fit beginning by loss of consciousness is a discharge beginning in some of the nervous arrangements in the highest centres of the brain. In this lecture we deal with cases in which there is little more than loss of consciousness, in which the peripheral effects are slight. Now, it is a singular thing at first glance, that it is in these very cases that we have not only symptoms referable to animal parts, but also symptoms referable to organic parts, as pallor of face, flow of saliva, limitation and stoppage of pulse. With either set of symptoms there may be stoppage of respiration with little spasm of limbs. Moreover, the effects are widely distributed, in severe fits universally distributed. Do not these facts show that the organic, as well as the animal, parts of the body are represented in the cerebrum and in the very highest of the cerebral centres?

These facts, as to implication of organic parts in epilepsy, find their parallel in the occurrence, in meningitis and in other acute primary brain diseases, of vomiting, constipation, retracted belly, irregularity of pulse. It is exceedingly important to note that from disease of "the organ of mind" we have such peripheral physical symptoms; the more so that whilst *they* are of great value in the diagnosis of acute primary disease of the brain, positive mental symptoms (delirium, &c.) are of little value, none, I think in diagnosis of such acute primary disease.

If it were admitted that the "organ of mind" was like lower centres, representative of nothing else than impressions

and movements, it would save us from many blunders. Surely there is nothing strange, *a priori*, in supposing that the higher and highest cerebral centres have the same fundamental *kind* of constitution as the lower centres in the pons, medulla oblongata, and spinal cord. From not admitting this, acute brain disease in its earliest stage is nearly always put down to fault in some non-cerebral organ, whose activity is particularly disturbed, directly or indirectly, by disease in that part of the brain which represents that organ. Is not migraine continually put down to digestive derangement? Is not ear disease overlooked in auditory vertigo, and the blame put on the innocent liver? Are not cases of tubercular meningitis and cerebral tumours in their earliest stages constantly ascribed to stomach and liver derangement? Now, to come back to our subject; because a patient's pulse is irregular during or just after an epileptic fit, the fit, supposing it to be slight, is put down to cardiac failure, even when the patient is suddenly seized when sitting reading or at dinner, turns deadly pale, and yet does not fall. Again, because a patient's fits begin with an epigastric sensation, described as nausea, or if there be belching of wind, it is not at all uncommon for the attacks to be ascribed to indigestion, notwithstanding that these stomach symptoms occur paroxysmally, and that ordinarily the patient may eat a big dinner and forget it, have after it no pain, no flatulence, no feeling of distension, no nausea, but go on digesting it unconsciously. So that if such a case is to be looked on as dyspepsia, it is paroxysmal dyspepsia causing loss of consciousness. Supposing the patient had dyspepsia, and supposing that he was invariably much more dyspeptic before each fit occurred, we ought to wonder why dyspepsia "caused" loss of consciousness, and caused it suddenly, and caused it transitorily. Trousseau mentions a case in which attacks of violent giddiness, with deadly pallor, occurred, after a sensation of pressure at the pit of the stomach, soon followed by vomiting. These attacks were put down to dyspepsia. The boy's father and his medical attendant refused to believe in Trousseau's diagnosis of epilepsy; the sequel of the case showed Trousseau to be right. Trousseau says, speaking of what he calls a visceral aura, that it escapes the observation of the physician all the more easily from its simulating other affections in a numerous class of cases. Herpin asks: "Is there anything more strange in the tonic convulsion beginning in the muscles of the digestive canal, than in its beginning in those of the hand and foot?"—a question very pertinent, since, as pallor shows, spasm in epilepsy frequently does begin, or occurs very early, in the muscles (muscular coats) of nearly equally organic parts (facial arteries). The fact is, as cases of meningitis, cerebral tumour, undoubted cases of epi-

lepsy show, that the organic parts are disturbed by cerebral disease in many cases in which visceral disease is supposed to disturb the brain.

We may fairly assume that the viscera and all other parts of the organism are doubly connected with the highest centres, if they are connected with the highest centres at all—nerve-fibres efferent downwards from centres to part of body; nerve-fibres afferent upwards from part of body to centres. Thus there is harmonious action, consent, betwixt the part and the centre; and when one gets out of order the other suffers. If, to speak figuratively, the brain (as in an epileptic fit) fixes the chest, the chest crams the brain with blood. If the brain stops or slows the heart, as in some cases of *le petit mal*, the heart won't supply the brain with blood. It is "You bother me, I'll bother you." There is a multiplication of effects. Hence a study of the several "visceral auras" is of great practical moment. I doubt not that when a discharge begins in a cerebral centre representing especially the stomach, when the fit begins by nausea, that centre, being in an unstable condition, is easily worried into discharging when the digestion is over-worked. Indeed, for more reasons than this, attention to the diet of epileptics is one of the most important things in their treatment, as Dr. Paget, of Cambridge, long ago insisted.

There are two other things referable to internal organs, which occur in slight cases, and more frequently in slight cases than in severe ones—viz., passing urine and fæces. It may be doubtful whether these are owing to epileptic spasm; they may occur after the fit, and be due to "loss of contrö." They occur after death. However, be these two things direct or indirect results of the paroxysm, they occur, at any rate, practically with the other symptoms. They are valuable helps to the diagnosis of epilepsy, especially in its slighter forms. I say in the slighter forms, for it is said that the urinary and fæcal discharges occur most often, as Herpin long ago asserted, in the *slight* attacks. Passage of urine is most common, and is a valuable help to diagnosis. If a child, but especially if an adult, passes urine in bed, epilepsy is *one* of the things to be carefully investigated for. If passage of urine occurs in a so-called "faint," or in a "giddy attack" in the daytime, there is probably epilepsy, almost certainly.

I will now make some general remarks on the diagnosis of epilepsy, and remark briefly on the question of pathology.

Perhaps one reason why on the occurrence of a few slight attacks there is a hesitation in carrying the evidence to its logical conclusion is, that the patient may, to all appearance, be in first-rate health. Unfortunately this goes, so far as we can yet tell, for nothing at all. Of course, before the first

attack, however slight, something must have been going wrong in the nervous centres. The nervous elements must have been become unstable before the first sudden explosion; a gun must be charged before it can be discharged. But what the first step to this is we know not. That nerve-cells somewhere are unstable is a truism. We infer that of necessity there is in every case some pathological process leading to increased nutrition, and therefore instability of cells. But what the pathological process or processes may be we have yet to learn; there is no morbid anatomy of epilepsy proper.

Possibly another reason for hesitation in diagnosing epilepsy is the absence of anything likely to be a cause of so serious a thing as epilepsy, and thus there is a tendency to fall back on the nearest most striking event, and to explain away the symptoms by fright, by dyspepsia, overwork, and so forth. We have not yet discovered the cause of epilepsy in any sense of the word "cause." We note carefully the facts of the above and other so-called causes, but until we find some morbid anatomy we are not justified in anything more than stating that epileptic fits occurred after or during this or that condition or circumstance.

As regarding both pathology and cause, I would repeat that there is a negative pathology; epilepsy proper, slight or severe, is rarely owing to gross organic disease, whilst epileptiform seizures frequently are. The changes are minute, as is evident enough by their having never been found. We ought to look into the state of each tissue. Is the change in epilepsy primarily arterial, and secondarily nervous, or is it a change beginning first of all in the nerve-cell? This leads to practical inquiry.

Another kind of evidence bearing on the diagnosis of epilepsy is the occurrence of nervous diseases in the patient's near blood-relatives. The fact that his relatives had hemiplegia or apoplexy from cerebral hemorrhage, is simply *no* evidence that he inherits a disease of the nervous system in the sense that the changes begin in the nervous elements of nervous organs, for the very plain reason that the pathological processes causing them are of a non-nervous kind; if they have a bearing, as I think they have, they point unmistakably to arterial or cardiac changes. But then the nervous affections in the epileptic's family may be such as chorea, neuralgia, or epilepsy proper. Here, again, since nothing is decided as to the pathology of any one of these affections, we cannot possibly say that they point to an inheritance of epilepsy, or tendency thereto, in the sense that they point to changes *beginning in* nervous elements. Why is it that we speak most confidently of the inheritance and interchangeability of those very diseases of which there is no morbid anatomy? I should not make these remarks were

they not to lead to a practical inquiry. The hypothesis in possession is that epilepsy begins in the nervous elements of nervous organs, and it is supposed to be supported by the fact that there are often in the patient's family persons who have that or other nervous affections, of which there is no morbid anatomy. I wish to put forward, again, another hypothesis—namely, that the cells suffer secondarily, as a consequence of arterial disease; that there is thrombosis or embolism of small arteries in most cases. I have held this opinion since 1864, and it first occurred to me on finding epileptiform seizures in patients who had valvular disease of the heart. I have since applied it to epilepsy proper. It may be said: “Your statement is only an hypothesis.” So is any other view which can be imagined of a disease which has no morbid anatomy. There is, at any rate, as much evidence—much more, I think—in favour of the “arterial” as of the “nervous pathology” of epilepsy. The practical matter is to search in the patient's family history for gout, rheumatism, cardiac disease, hemiplegia, and apoplexy (both usually arterial affairs), as well as to inquire about nervous affections.—*Lancet*, Jan. 25, 1879, p. 111.

20.—CASE OF CONFIRMED EPILEPSY, WITH IRREGULAR AND PAINFUL MENSTRUATION, SUCCESSFULLY TREATED BY LECLANCHE'S CONTINUOUS CURRENT.

By Dr. A. S. MYRTLE, Harrogate.

[Dr. Myrtle gives us an interesting case of epilepsy in which he tried various remedies, but none were so successful as the continuous current. He says:]

In March, I proposed trying the effect of the continuous current of Leclanché; and on April 20th I commenced with five cells. I passed the current from the lower spine through both ovaries and uterus for fifteen minutes each morning, and gradually increased the strength as she could bear it to twenty cells. She became unwell within a week of her expected day, and continued so for three days. There was no nerve-disturbance before or after the period. A few days after she was better, I again applied the current, giving up to thirty cells; and the flow made itself visible the very day on which it ought. In twelve hours, it ceased; so I again had recourse to the battery, and in two days all came right, and the discharge was greater than it ever had been, and continued for four days. She remained perfectly free of all epileptic symptoms, and her temper and health were better than ever. I allowed seven days to pass before applying the current; then I made use of it, going up to forty cells.

A week before the expected time, I made her sit daily for

fifteen minutes in a hip-bath with a solution of hydrochlorate of ammonia, and passed the current through the water; and on the very day they ought the menses appeared, but again ceased for two days, to reappear and pass through their natural course as on the previous occasion. Again there was perfect freedom from all epileptic signs; so much so that on the second or third day she was out on horseback.

During the time I used the Leclanché I never omitted the bromide, as her system seemed to have become so used to it that she could not or would not do without it. Of course, three months are not a sufficient test of the power of any remedy; still I think the result is so far satisfactory as to be worthy of record.

One fact I must mention, and that is, that under certain conditions the nerves seem almost insensible to the action of the current; at other times, they resent its mildest application. Another is, that, in giving a bath, care must be taken that the walls of the bath do not abstract the current from the water and the body of the patient; the best plan being to use a copper wire covered with India-rubber solution for the pole inserted into the water, and for the other any other electrode to be moved over the spine or any part of the trunk of the patient either by her own or attendant's hand. The application of this battery can be put in practice by any woman of common observing powers after a single demonstration. For medical purposes, I recommend a forty-cell battery, as it happens very often that the patient can bear that number of cells quite as well as ten or twenty.

I have just learned of the success of the same treatment in two cases—sisters—one aged 19, the other aged $17\frac{1}{2}$. The elder has been irregular always, sometimes missing one or two periods. She has suffered from severe headaches, has had occasional faintings, and her spirits have been greatly depressed; and at intervals she has been unlike herself in disposition and conduct—in fact, epileptiform. She has had all sorts of emmenagogues given her without any result. I ordered her a Leclanché's battery of forty cells, explained to her mother how and when to use it, and she writes me to say that it "has answered perfectly and produced the desired result," not only in the case of the elder girl, but also in the case of the younger, who seemed perfectly healthy, but showed the same disposition to irregular and painful menstruation as her sister. Having only received the above intelligence to-day (August 5th), I regret I cannot furnish further particulars. In the case of the elder girl, I forgot to say that the "period" was not only irregular, but, when it did appear, gave much pain, and necessitated rest in bed for twenty-four hours.—*British Medical Journal*, Nov. 16, 1878, p. 718.

21.—ON A CASE OF HYSTERO-EPILEPSY.

By Dr. THOMAS ANDERSON, Rosewell, Edinburgh.

M. H., aged 18, had always enjoyed good health, although, a year ago, she broke her arm. She was first taken ill while at her work. She suddenly felt a pain in the left breast, which shot up to her head; then a rushing sound in her ears; everything became dark, and she fell. When she returned to consciousness, she found herself in bed. These attacks recurred regularly every half-hour until I saw her two days afterwards. At the time of my first visit, she lay in bed unconscious, and making regular and rhythmical movements with her arms and legs, and also with the muscles of the face; the eyes were squinting and turned upwards. My first impression was that what I saw was the clonic stage of an ordinary epileptic seizure. The temperature, however, was only 98·2 deg., and, on pressing downwards in the left iliac region, she became sensible immediately, and was smiling as if nothing had happened. On resuming the pressure in the same situation, she screamed and became unconscious again, the same clonic movements having also returned with all their original intensity. The pressure again restored her to sensibility. I instructed her mother how to proceed on the occurrence of another fit, and left. Next morning, I was sent for hurriedly, the report being that, on coming out of a fit, she was unable to speak. On visiting her, I found that this was the case; for, even when severely tested, she could not utter a syllable, although she protruded the tongue, and moved it in every direction in her efforts to articulate. In addition, there were loss of hearing on the left side, achromatopsy, and loss of smell and taste, anæsthesia of the left arm and leg, and contracture of the legs. I need scarcely recapitulate all the details of these symptoms, which were those usually noted in this disease. On applying several metals—zinc, iron, copper, silver, platinum, &c.—there was no effect produced; but, on placing a sovereign to the left temple, she gradually recovered the perception of colour in the reverse order (as I afterwards found) of its loss. The same metals were then applied to the legs, first to the left and then to the right; sensibility and the power of movement returned in twelve minutes. Again, on placing the sovereigns under the tongue, her speech returned in ten minutes; but *gasping* efforts to speak began in five minutes after the application of the gold, and these continued until she took a long inspiration and said, “There it is now.” Her speech was lost again in half an hour, and each time the gold was applied it was observed that the time taken to recover the sense or faculty was longer. Chloride of gold and sodium were administered internally, and

the fits became gradually less frequent and of shorter duration. Occasionally, after a seizure, there were anæsthesia and analgesia; and again, after the next fit, sensibility sometimes returned.

In two weeks after my first visit, the aphasia was still present; but she stated that she had been able to speak for short intervals, and, on the occurrence of a fit, she had lost the power again. I had the gold applied for two hours; it had no effect, and the patient made fruitless efforts to speak. I removed the coins, and applied the continuous current of a battery. In a few minutes, the aphasia disappeared, but not at once; for she had tried to speak several times for the sake of having the battery removed. After this date, electricity was applied whenever there was any loss of sensibility, and with the invariable result of restoring it in a few minutes. At this period, with the view of ascertaining what part, if any, electricity played in the action of the metal, I made some experiments with a delicate astatic galvanometer and Wheatstone's bridge, indicating 1 Ohm in 50,000, using a chromic acid battery. I found that the current given by the sovereigns was so small as scarcely to deflect the needle of the galvanometer. The "resistance" in this patient was very great, and the least amount measured was 142,000 Ohms, when the electrodes were placed over the radial artery at the wrist. Now, in a number of healthy persons, I have never found this resistance amount to more than 30,000 Ohms, which is very rare, and in most of them it never was more than 7,000 Ohms.

During the second month after the first accession of symptoms, the patient had only one fit, the catamenia (which had been absent for three months) returned, the anæsthesia, &c., disappeared altogether. It is now five months since she took any medicine, and during that period has enjoyed very good health.

In the recent number of *Brain*, Dr. Hughes Bennett states, in concluding his article on Metalloscopy and Metallotherapy, that "it must be admitted that the above observations tend to show that the beneficial changes resulting from the application of metals to the skin are due rather to the mental effects they produce than to any special virtues emanating from the metals themselves." Now, in the foregoing case, as in M. Charcot's, when the patient was in an unconscious state, pressure was exercised in the iliac region, what part had "expectant attention" or "mental emotion" in restoring her to consciousness? And how could a comparatively uneducated person knowingly give the colours in strictly mathematical order, when the perception of colour returned? In this, as in Dr. Bennett's and in some of Professor Charcot's cases, as I have stated, the patient

did certainly occasionally recover the lost sensation without the application either of the metal or the galvanic current. At first sight, the action of the metal might be put down as an electric one; but, when the "resistance" was so great in this patient, how could such a weak current as that given by the gold affect her, and also, as M. Charcot says, why did the application of the other metals, which also give rise to electric currents, have no effect? I am more inclined to believe in the theory of a specific action of the selected metal, of the nature of which we have no knowledge. Although, in this case, the insertion of the needle into the anæsthetic parts was not followed by bleeding, yet, in the wards of La Salpêtrière, I have frequently seen slight hemorrhage follow, and that, too, without reproducing sensibility.

In conclusion, I may state that all the symptoms in this case were noted, and the experiments performed, in the presence of competent medical witnesses.—*British Medical Journal*, Feb. 8, 1879, p. 186.

22.—TWO CASES OF HYSTERO-EPILEPSY.

By Dr. WILLIAM JOSIAH SMYLY, Assistant-Physician to the Rotunda Hospitals, Dublin.

Few subjects which come within the domain of medicine have of late excited greater interest than that of hysteria-major, hystero-epilepsy, or epileptiform hysteria, as the affection is variously denominated. The number of cases published in our journals during the past twelve months, and the hot contests between those whose opinions differ upon the various complex questions which the subject suggests, sufficiently attest the truth of this assertion. It is not, I imagine, as has been stated, "a disease of extreme rarity," for the fact that so many cases have been assembled at one time in a single institution—namely, the Salpêtrière Hospital in Paris, that so many have been recorded in these countries during a single year, and the fact that two examples have fallen within my own personal experience, seem to contradict the assertion. In its symptoms the affection is most appalling to onlookers. Its course is very uncertain, and does not, according to Professor Charcot, tend towards recovery. In some instances, as in one of my own cases, and in some recorded in Professor Charcot's lectures, it even terminates fatally. The method of treatment proposed for its cure is one of the most extraordinary and novel in modern therapeutics, and claims more from our credulity than the notorious system of Hahnemann. Here we have only to discover by experiment the metal to which the patient is susceptible, and its internal administration is the most certain

means of cure—not only so, but we have also the means of telling whether the cure is complete or only apparent—for, by placing a dish of the same metal upon the forehead of your patient, the symptoms will, in the latter case, immediately reappear.

Although I must confess to being sceptical on the subject of metallotherapy, and incline to the opinion of those who consider that the results obtained by some are owing rather to mental than to physical causes, still, as Dr. Sigerson has said in a recent paper on this subject, “it is right to remember that we have here not simply the assertion of *an* investigator” to deal with, “but the labours of a commission appointed by an eminent society to examine and test his statements.”

The symptoms of the disease are well known through the admirable translations of Professor Charcot’s lectures by our fellow-citizen, Dr. Sigerson, in which, and in his original papers, will be found, I believe, almost all that is at present known upon the subject.

I shall not, therefore, occupy time by further preamble, but proceed to the account of two cases which have come under my personal observation.

The notes of the first case were taken in the year 1876, before I had any knowledge of the works of Charcot or Burq; and although wanting in many symptoms, which might have been discovered had they been looked for, are still interesting, I think, as the observations of a mind unprejudiced by preconceived opinions.

Case 1.—Miss G., aged twenty-six, menstruation always irregular; but otherwise, until three years and a half ago, she enjoyed perfect health.

As the history of the case extends over so long a period, I shall content myself with the description of a single series of convulsions and a short *résumé* of the development and progress of the disease.

Having, for some time, wished to be present during an entire attack, and having had a free day on the 10th April, 1876, I called at the patient’s lodgings at half-past eleven in the forenoon. I found her, having just finished breakfast, lying upon a sofa, perfectly conscious and capable of conversing, but interrupted frequently by what seemed to be laryngeal spasm, which was accompanied by a peculiar noise, something like a dog coughing. She also vomited occasionally, the quantity brought up each time being small, and the act unattended by nausea.

At noon a wet cloth was placed over her eyes, when the barking sound immediately ceased, but she began to clear her throat violently and to expectorate copiously. Moaning in a

piteous manner, she soon lapsed into a state of apparent unconsciousness. The left fore-arm was rapidly pronated and supinated, and the corresponding foot synchronously flexed and extended.

How long this might have continued I cannot say, for it was suddenly cut short by her aunt tickling the side of her face. At this she shuddered, as if frightened, and started into a sitting posture, her knees drawn up under her chin, and her arms crossed under her thighs. She turned her face towards the back of the sofa; and a chair, with a pillow upon it, having been placed behind her, she began to kick against the back of the sofa, slowly and gently, with the regularity of a machine.

Having continued thus for upwards of an hour, she got lengthwise upon the sofa, down towards the lower end, her legs hanging over the arm of it. Suddenly she flung her heels over her head, assuming very much the position of a Chinese juggler when balancing something upon his feet—standing upon the nape of her neck, the body and legs rigid and perpendicular to the horizon, and the arms crossed behind her back, so as to afford a broader basis of support. This position was maintained for fifty seconds, when she came down suddenly and heavily without flexing a joint. The legs were then thrown backwards and forwards over her head about ten times. She now assumed a different attitude, with flexed thighs and extended legs, the toes almost touching the sofa above her head, both upper and lower extremities being agitated with clonic spasms. These two positions—the first attended by tonic and the second by clonic spasms—with the violent to-and-fro movements interposed, were regularly alternated for half an hour, each being maintained, however, for a longer period each time, until at last she stood two minutes and a half upon the back of her neck; then, throwing her feet one hundred and ten times over her head, she lay exhausted and gasping for breath.

Having rested a minute, she began again, but the paroxysm assumed quite a different character. The legs, instead of being closely pressed against each other, during the to-and-fro movement, were twice crossed during the ascent, and twice during the descent, and her body being at the same time rotated from side to side, the feet were shot out right and left alternately. The intermissions were not attended, as in the first stage, by complete relaxation, but the upper extremities were spasmodically flexed, the fingers on the hand, the hand on the fore-arm, and the latter on the arm.

At three o'clock the paroxysm became most furious; the feet were thrown backwards, forwards, and from side to side, the legs alternately crossed, extended, flexed; the hands entangled in her clothes were wildly flapped on either side; her face

hideously contorted, the under lip thrust out, and the saliva blown in foam from her mouth; respiration laboured and rapid, and an incessant barking sound proceeded from her throat.

After a few moments' rest she covered her face with her hands and wept piteously; then, raising herself on her elbows and crawling to the upper end of the sofa, laid her head against the side and passed into a state of syncope.

Soon, however, the convulsions returned, and she repeated the performance as described, and this she did a third and fourth time with unvarying precision. After the final paroxysm she rested for a longer time, but instead of beginning again she grasped her throat with her right hand and rapidly turned her head with a semi-rotatory motion, the hair standing out around it like the strands of a trundling mop; she then tossed it to and fro, knocking it against the sofa with horrible violence. She became quiet, opened her eyes, gazed fixedly at one corner of the room as if entranced, then, shooting out her hand to grasp at something, she became conscious.

She seemed surprised to see me there, and said she was tired and thirsty; cold water was brought, with which she rinsed her mouth, but dared not swallow any. It was now eleven o'clock at night, and the performance began at noon.

Calling next morning, I found that she had passed the night in a state of trance, so that she could not be removed into bed.

Calling again in the evening, I could hardly believe that she was the same person whom I had seen that morning or the day before. She surprised me by her cheerfulness, knowing, as I did, that she had not eaten for thirty-six hours, and that the greater part of the scanty meal which she had then taken had been vomited soon after. She complained of pain in left ovarian region, which was also tender; in the epigastrium and left side of the face under the clavicle, and at the angle of the scapula of the same side. These symptoms had been constant for months. If the tender spot at the angle of the scapula should happen to be pressed, even by a wrinkle of the bed clothes, she would pass at once into a cataleptic condition, remaining so perhaps for hours. There was a spot also on the top of her head which must not be touched; it was about the size of a florin, and the hair growing upon it was never brushed or combed, for should it be touched she would at once begin the spasms as already described, which would continue for eleven hours. I once touched this spot by accident, in the middle of a fit; she immediately became conscious, but began all over again, with extraordinary precision, so that the fits lasted fourteen hours that day. These phenomena were also excited by the act of deglutition.

Could she have subsisted without swallowing, she would have had no spasms. For a fortnight that she was supported by nutrient enemata alone, she had not a single attack.

Sometimes she complained of severe pain in the left side of her face, which soon would become hot, red, and swollen; and, after a period varying from a fortnight to six weeks, these symptoms would subside.

The assigned cause of this illness was shock to her nervous system by the sudden death of her father, to whom she was greatly attached. This occurred while she was standing by his bedside. She seemed unable to leave the spot where she was, and was at last carried to her bed, where she "swooned" away, and did not recover consciousness until the next day. Shortly after this, during a violent paroxysm of sobbing, her inspiration assumed a loud crowing sound. She soon acquired a habit of tapping the ground with her left foot, which subsequently became paralysed. She, however, partially recovered the use of the limb; though at the time I knew her, she described it, as well as the hand and arm on the left side, as "dead feeling," though occasionally she could use the hand in sewing.

Believing that her disease was chiefly laryngeal, she came to Dublin in 1873 to consult my brother, who found the larynx healthy. She was also seen by Dr. Kidd, who declared that, with the exception of tenderness in the ovarian region and some abrasion around the os uteri, the generative organs seemed to be sound.

From the first nervous shock the menses were not seen; her friends were, however, occasionally alarmed by the presence of blood in the urine, which was probably vicarious; other urinary symptoms, such as dysuria or suppression, were occasionally observed.

The therapeutic agents from which she derived most benefit were blisters. There was no doubt that she was better so long as a blister remained open. For some time chlorodyne, and afterwards croton chloral in large doses, had some effect. Chloroform stopped the contractions, but as soon as it was discontinued, they commenced again exactly where they had been interrupted. Bromides, foetid gum resins, and valerian, were useless—the cold bath, electricity, and the strait-waist-coat, positively injurious.

In December, 1877, whilst I was absent from home, violent diarrhoea set in, alternating with vomiting, both of which resisted treatment. She died in perfect consciousness, complaining of pressure on the chest, and violent pain in the bowels. There was no blood or symptoms of dysentery.

The following points in the case I would direct attention to as resembling the symptoms in Professor Charcot's cases:—

1st. Ovarian hyperæsthesia, with numbness of the same side.

2nd. The convulsions divided into stages:—

a. The epileptiform, which was badly marked.

b. “Phase des grands mouvements,” which could not have been more extravagant.

c. “Phase des attitudes passionelles,” represented, I think, by the period when she raised herself upon her elbows, retreated towards the sofa end, and covering her face wept piteously.

3rd. The sensitive regions, or epileptogenic zones, one on the top of the head, and the other somewhere in the pharynx or œsophagus.

The assigned cause, a sudden fright; and the action of chloroform, controlling for the time, but not curtailing, the movements.

The second case is still under observation.

Case 2.—The patient, Mrs. T., aged twenty-two, is a married woman, and has had one child. She has been married about two years, and her relatives informed me that she is of an exceedingly nervous and passionate temperament. She has been hystero-epileptic for about a year; cannot assign any cause for the origin of her disease, but says that “the fits come on when she is annoyed.” She was treated for some nervous affection in the Mater Misericordiæ Hospital when a little girl.

She first came under my notice amongst the out-patients attending the Rotunda Hospitals’ Dispensary about the middle of last November; was then suffering from an abscess in the left mammary gland; she also complained of “nervousness” and uncontrollable shaking of the right arm. Her eyes having been covered, a needle was thrust into the back of the right hand so that it bled. As she evinced no sign of pain, I asked did she feel anything touching her? She said “no.” The left hand was then pricked, but she immediately withdrew it with an exclamation, and forcibly uncovered her eyes. I tried to persuade her to allow me to prick the right hand again whilst her eyes were open, but she would not permit it. I found, on further examination, that there was complete anæsthesia on that side, affecting the skin and mucous membranes, and almost complete analgesia. The special senses were also affected—taste and hearing being absent—and there was well-marked acromatopsia. When the left eye was blindfolded she described brown as black, and red as brown; a green book with gold letters she said was white with black letters. There was ovarian tenderness on the right side.

I have never been present during a convulsive attack, but

they have been described to me by her relatives, and also by the midwife who attended her in her confinement—the latter, who is an intelligent woman, said that at first she thought that they were puerperal convulsions, but that she kicked about much more; after the fits she slept profoundly for a long time.

The patient herself informed me that she knew by a peculiar sensation rising from her stomach when a fit was impending, and she immediately lay down.

I was not able, therefore, to employ personally the method proposed for controlling these paroxysms, by pressure in the ovarian region, but I endeavoured to teach her sister how to employ it; and she told me that on two occasions that she had done so, the attack was immediately interrupted, but as soon as the pressure was removed the symptoms immediately reappeared.

I completely failed in all my attempts to cause a transference of the anæsthesia to the opposite side. I first tried the effect of a large electro-magnet, which was kindly lent to me by Dr. Coppinger, of the Mater Misericordiæ Hospital, for the purpose. The patient being blindfolded, the poles of the instrument were approached to the skin of the forearm, but although we waited a considerable time and tried them at various distances, there was no result. A solenoid, formed of an insulated wire coiled in a spiral and connected with a single galvanic cell, was afterwards placed upon the index finger of the affected hand, but still the result was negative.

On the 20th December I first mesmerised my patient. This was effected by holding a bright object within about six inches of her eyes, and bidding her look at it intently. Soon her eyelids drooped, the head fell forward upon her chest, and she appeared to be fast asleep. The limbs were quite relaxed and anæsthesia complete, a needle being driven through the web between the fingers of either hand without causing pain. I now blew suddenly into her face, hoping to arouse her, but instead of this she came rigid. I raised her arms and they remained outstretched—she was evidently in a cataleptic condition. I then placed the electrodes of a Brammer's battery in her hands; this I used at its maximum power, but it was some moments before it had the slightest effect. She suddenly became conscious, however, and looked round as if surprised and startled.

Having read Dr. Carpenter's paper in the British Medical Journal, in which he describes the wonderful effects produced by a portmanteau key upon an hysterical patient, I determined to repeat his experiment. Having placed the ring of a pair of dressing forceps upon the first finger of the anæsthetic hand, I informed the bystanders that sensation would soon return, and such was the case. I then changed it to the second finger,

saying it will now depart again, and so it did. I have since, on several occasions, tried to repeat this experiment, but have not succeeded.

The symptoms, as above described, are not constant—on one or two occasions there was no anæsthesia; most of the other symptoms were, however, present, and the patient was easily mesmerised, after which hemi-anæsthesia remained for some time.

On the 24th of January she came to me with symptoms of paralysis of the facial nerve, and protrusion of the tongue, which she declared she could not keep in her mouth. I have searched in vain for sensitive regions, but have not found any. —*Dublin Journal of Medical Science*, March, 1879, p. 200.

23.—A REMARKABLE CASE OF HYSTERIA.

By Dr. JOHN S. BRISTOWE, Senior Physician and Lecturer on Medicine at St. Thomas's Hospital.

Hysterical Catalepsy: Mania: Double Consciousness, &c.—A healthy-looking, interesting, and accomplished girl was, without any obvious reason, attacked, in September 1873, with pains in her chest-walls simulating those of intercostal rheumatism. They were supposed to be hysterical, and had subsided at the end of a week. But after a couple of days, again without apparent cause, she passed almost suddenly into a curious cataleptiform condition. Her muscles were perfectly rigid; her arms and legs and head and neck being extended so that she could be carried or lifted like a statue from one place to another; but her hands were firmly closed, and her feet similarly affected, so that the toes were flexed into the soles of her feet. The joints generally were so firmly fixed that no force that her medical attendant, Dr. Simpson, of the Old Kent Road, felt justified in employing, availed to move them. The only exceptions were the shoulder-joints and those of the head and neck. The arms, in fact, could be moved readily at the shoulders, and remained for an indefinite period in any position in which they were placed. The movements of the head and neck I will describe presently. She appeared to be entirely devoid of feeling, perfectly deaf, and her eyes were fixed, but she was not wholly insensible; for, although she took no notice of anything, or of anyone else, she always showed signs of pleasure, and moved her head from side to side, when Dr. Simpson came in the line of her vision. Yet she took no notice even of him when his image fell on the lateral parts of her retinae. Her teeth were clenched, and she did not attempt to speak or to take food. But Dr. Simpson soon found that, on passing his finger backwards between her teeth and cheek, and

then inserting the tip behind the teeth between the upper and lower jaws, the spasm of the jaws almost immediately yielded, and he was able to administer spoonfuls of milk. But there was still some difficulty in swallowing, and it was often returned, in part by the nose. He had to feed her four times a day. At the end of about a month, she recovered almost suddenly; and when her hands and feet opened, the fingers and toes were found to have made deep depressions in the palms and soles respectively.

In December, her friends determined to take her to Brighton for the benefit of her health. Before she left, Dr. Simpson paid her a complimentary visit; and immediately after this she observed to her parents that, if she were taken ill there, she would have only Dr. Simpson to attend her. She left home on the Thursday, and on the following Monday morning a telegram reached Dr. Simpson requesting him to come down to her at once. He arrived at Brighton in the evening, and found her again cataleptic, and very nearly in the same condition as that from which she had recovered two or three weeks previously. Dr. Simpson then learnt the following facts about her, some of which were important, and others at any rate strange. It appeared, that about 11 o'clock that morning she began to experience a sense of numbness and rigidity; and being quite sensible at the time, she requested that Dr. Simpson should be at once sent for. Her parents went into another room, discussed the question, and decided not to telegraph (at any rate at once), but to tell her that they had telegraphed. They returned to the room and told her that they had sent her message. But she at once taxed them with their deceit; and the result was that the telegram was sent off. At 12 o'clock, she said to her parents, "The telegram has reached Dr. Simpson's house, but he is not at home, and will not reach home until two." At two, she observed, "He has now got the message, but he is hesitating whether he shall come or not." Later on in the day, she said, "Now he has started," and about an hour or so afterwards she called out that his train was being delayed at Horsham, and that it would arrive at Brighton half an hour late. Now, all these assertions of hers were, Dr. Simpson asserts, absolutely true. He was out at twelve, when the telegram arrived; he did not reach home until two; and then he had to go to the London Hospital and get a *locum tenens* before it was certain that he could go to Brighton that day. His train, which she had timed accurately, was delayed half an hour at Horsham, and arrived at Brighton half an hour late. Her condition, as above remarked, was now, as nearly as possible, what it had been previously: she was rigid, deaf, almost, if not quite, insensible to pain, and showed pleasure

when Dr. Simpson came into her line of vision. But now the sight of other persons, which was formerly unheeded by her, threw her into violent convulsions. She, in fact, took her father and mother and other persons for wild beasts, usually lions and elephants; and, on catching sight of them, immediately, with a loud scream, passed into an epileptiform attack, in which she lay for about a quarter of an hour, moving her head backwards and forwards, and with her face and eyes turned powerfully to the left. There was no foaming at the mouth, and no biting of the tongue, and she came to with a deep-drawn sigh. On the following Wednesday, she was brought back to London, being lifted from her bed to the carriage and from the carriage to her bed "like a rod of iron." She continued in this condition for eight or ten weeks: rigid, insensible to pain, deaf, suffering from very frequent fits, all brought on in the way described, and taking food, in the form of milk, from Dr. Simpson only. It may be observed that, in order to avoid sending her into a fit, her mother used, when she wanted to pass her evacuations (of which the signs were always easily recognised), suddenly to throw a sheet over her head, and then she was lifted by her head and heels and placed supine over the bed-pan. During the greater part of this time she made no attempt to speak; but at the commencement, and also when she was convalescent, she would occasionally, on recovering from her fit, refer to the elephant or lion she had seen. During the whole of this period, the fits came on several times a day.

Towards the end of the time referred to, the rigidity gradually disappeared, commencing from the hips. But she now began to suffer from extreme pain and tenderness in her limbs, so that she could not bear to be moved or even to be touched, and would frequently scream with pain. The soreness and tenderness, however, gradually subsided; and before long, she fell into a strange condition, in which she continued with little or no variation for the next two years. She could now move her limbs freely, and, indeed, seemed to have much muscular power, yet was unable to rise from her bed, or even to be fed, without assistance; and even then, the exertion was usually attended with an attack of profound syncope. She could feel, but there was marked insensibility to pain, and she was absolutely deaf. On the other hand, she was closely observant of all that was going on round about her; was voluble in her talk, her vocabulary being peculiar; and would often sing; would never take food from any one of her immediate friends or relations; was vindictive, biting if she had the opportunity, and concealing sticks or knives under her pillow, and, at any rate, striking with the stick if she had the chance. For a long time, Dr. Simpson was the only person whom she

cared to see, or whom she would obey. As soon as she saw him in the room, she would tremble with pleasure and eagerness, hold out her arms to him, fondle his hand in hers, and begin to talk volubly. She would first say, "paws," which meant shake hands, and if he did not shake hands, she would become sullen, cry, and refuse food until he did. Then she would say she had a lot to tell him, and would recount all that had occurred since his last visit. His visits, which were for the purpose of feeding her, were made four times a day. She would only take milk; would always insist on leaving some in the basin; and the more she left the better, saying she had the best of the bargain. Generally, when she commenced, she would hold up her fingers, signifying by that, that she would only take ten spoonfuls, and generally that was all that she could be prevailed upon to take; but occasionally, Dr. Simpson, by holding up two fingers, would persuade her to take two spoonfuls more. But then her refusal was absolute. She was not particular as to the size of the spoon, and consequently the largest possible table-spoon was employed ultimately. She never took more than three pints of milk a day, and no other food whatever. After a few months, Dr. Simpson, by gradually introducing to her his partner and his assistant, managed to prevail on her to allow herself to be fed in turn by them. During this time, it was intimated to her that Dr. Simpson wanted articles for a bazaar; and for a period of four months she devoted herself to dressing dolls, of which she dressed more than fifty, and making ink-markers. The dolls were very tastefully got up, and some were arranged in groups, illustrative of the old woman who lived in a shoe, and the like. During the whole of this time, she had a strong antipathy to her father and mother and other relatives, and did not even seem to know who they were. She knew them, however, from one another, and gave them names. One sister she called a "dog," another sister a "fox," her mother was "that one," her aunt was "the other one," and another friend was "the other t'other one." I do not think that her father had a name, but she disliked him more than all the rest, and for a long time, as her fits were gradually disappearing, his presence alone caused them. From first to last she had between one and two hundred fits.

At the end of about two years from the commencement of this condition, her mother one morning heard her heave a deep sigh, and saw her head drop suddenly on one side upon the pillow. She thought she was dying, rushed up to her and said, in alarm, "Do you know me, Edith?" when the daughter replied "Yes," in a scarcely audible whisper. This was the first indication of hearing, and of recognising her mother,

which she had manifested. She remained in this condition for a few minutes, and then suddenly passed again into her maniacal state. From this time forward, she would have, nearly every day, one of these lucid intervals. At first, they would last for about ten minutes, but gradually increased in duration, so that, at the end of six months, they would often last from half-an-hour to two hours. These lucid intervals always came on in the manner above described, and always ended suddenly. They were always characterised by the same phenomena, and these, it will be observed, were in many respects the exact converse of those which marked the maniacal state. While in them, she appeared to preserve all her natural qualities of mind; was timid and affectionate, knew and appreciated her father, mother, other relatives and friends, and moreover, seemed to have no recollection of what took place during her alternative condition. But she was in a state of utter bodily prostration. She spoke in a whisper; she kept her eyes closed with tremulous eyelids, but could see when they were opened for her; and her hearing, which in her other state was absolutely lost, was now preternaturally acute. Indeed, any slight shock, and especially a slight but sudden noise, would render her at once maniacal. On the first occasion on which Dr. Simpson saw her in a lucid interval, he conversed with her quietly, asked her to put out her tongue, which she did, and then invited her to take food, which she declined, but in a quiet, lady-like way, and simply because she was not hungry. He asked her also if she knew him, and, although she knew her father well, she told him she did not recognise who he was. He then tried to feed her with a spoon, when she suddenly opened her eyes, was excited as usual at his presence, at once greeted him with "Paws, doctor," and insisted on shaking hands with him, and said she would have her "bitings," which was the term she applied to her meals. The lucid intervals gradually increased in duration, so that at the end of another four or five months, they sometimes continued for days together. Moreover, as these increased in length, the others diminished, until at length they constituted what might be termed mad fits of a few minutes' duration only. It must be added, that the extreme bodily prostration of her lucid periods gradually lessened, and that by degrees the power of opening her eyes, of speaking aloud, and of taking food without going off into a maniacal paroxysm, were restored during their progress. She had improved thus far, but was still quite unable to walk, by November last. At that time, her parents determined to move to another house. She was removed, under the superintendence of Dr. Simpson, in an open phaeton, placed upon a bed which was laid diagonally across the seats.

In the attempt to remove her, she went off into a cataleptic state, in which she remained all that night, and until the afternoon of the next day; she then became maniacal and continued maniacal for two or three days more; then this condition also subsided, and she was restored to the state in which she had been just prior to her removal. She gradually improved in health and strength, but continued unable to walk until she was taken to the Isle of Wight, in July, 1877. Here she recovered perfectly; and, early in October, 1877, on her return to London, she walked without assistance to Dr. Simpson's house, and reported herself well.

I may add that, while the alternative conditions above described were in progress, she appeared, while in the one, to know absolutely nothing of what had occurred in the other, but to retain a vivid recollection of all that had happened to her in the previous periods of the same condition; that, although her diet for more than two years consisted of somewhat less than three pints of good milk daily, there was no appreciable emaciation; that her excretions (with the exception that there was a tendency to constipation) were normal, and that she menstruated regularly; and that no bed-sores or excoriations formed. I had the opportunity of seeing her once or twice, and was present on one occasion when she passed from her maniacal to her comparatively sensible condition.—*British Medical Journal*, Feb. 8, 1879, p. 184.

24.—ON HEMIANÆSTHESIA AND ITS CURE.

By Dr. SAMUEL WILKS, F.R.S., Physician to Guy's Hospital.

The subject of hysterical anæsthesia and its cure after the application of metals seems to have divided the profession into those who find an explanation for the result in galvanic agency and those who, discarding this as purely speculative, connect it in some way with a mental operation. Neither view, to my mind, will satisfactorily account for the phenomena; nor do I think it probable, in the present state of our knowledge, that an explanation can be looked for. We cannot unravel the mysteries of a very intricate piece of mechanism by watching it; discoveries have never been made by any such method. Researches are instituted to solve simple questions, and thus step by step, by discovering new laws, we apply these in explanation of complex conditions which had hitherto been unintelligible. No one, by looking at the brain or nerves, can tell the nature of the forces operating; and, if he conjecture that they may be electrical or of the nature of those forces which have lately been observed in the telephone or microphone, it must be remembered that we have gained a knowledge of

these forces in the laboratory. It is probable that, if we could at the present moment be told the nature of nerve-force, the very terms of the expression would be foreign to us. We are, indeed, in the position of not knowing what kind of force is produced in the brain and cord, nor the mode in which it is transmitted. Our knowledge is almost limited to the facts that an act of the will can move our muscles, and that a galvanic current can do the same. We, therefore, consider that every observable change in the nervous or muscular system must be due to one or other of these causes.

In cases of hemianæsthesia, it is very clear that other substances than metals will induce a cure; but, on this account to attribute the results to the will or some mental operation, affords us no explanation of the nature of the phenomena; for, if it be granted that an effort of the will is sufficient to restore movement to a paralysed limb, and bring back not only common sensation, but the activity of the special senses, which had been previously lost, we cannot understand how any state of the will can account for the production of the original symptoms. The case is often argued as if the hysterical condition were one of shamming or feigning, so that, if a woman have paralysis of a limb, it is the same thing as if she did not choose to move it; but a mere unwillingness or negative state of mind is certainly not the same as hysteria. Any of us may declare that we cannot move an arm or leg, and at the next moment change our mind and move them; but, in the hysterical condition, this is rarely possible; for the will has gone. Sir James Paget says rightly that the very essence of the disease consists in the patient not being able to "will." To rouse the will constitutes the cure. Thus it happens that an hysterical woman will ask the doctor to aid her in making the effort; and, if she cannot accomplish it herself, a sudden shock or fright will rouse her nerve-centres sufficiently for her to throw the requisite force into the limb. If there were any doubt as to the hysterical condition being much more intricate than that dependent upon a want of volition, the accompanying hemianæsthesia and loss of the special senses would show it; for the latter could scarcely be due to any effort or want of effort on the part of the patient. She can deceive and declare she is blind when she is not so; but she cannot produce the hysterical amblyopia.

To me it seems clear that, in these cases, the brain, as a whole or in part, has ceased to act or perform its function; no other explanation appears to me at present sufficient to include all the phenomena; for we notice that there is paralysis of motion, sensation, and of all the special senses and therefore the function of the brain must be in abeyance, somewhat after the manner of sleep.

My principal object in the present communication is to draw attention more especially to the fact that an injury or blow is often the starting-point of the attack; this is an additional factor to be considered in discussing the nature of hysterical paralysis, and is of great interest in connection with the subject of localised lesions of the brain. Thus a patient who lately left Guy's Hospital attributed her symptoms to a fall. She had been under the care of Mr. William Toulmin, of Clapton, in consequence of her having fallen over a bottle and struck her back, on which followed weakness of the right side, loss of sensation as well as of hearing, seeing, &c. On suspicion that her back might have been injured, Mr. Toulmin put her on a Sayre splint and sent her to the hospital. She displayed all the usual phenomena of hemianæsthesia, and was improving under the latest method of metallotherapia when she left us. An exactly similar case to this has been reported to me, in which the whole of the phenomena which Charcot describes were produced by the application of metals, and in this girl the paralysis ensued from a fall on the back. The readers of Brodie will remember how, in his valuable work on the joints, he with great acumen points out the fact that hysterical and other nerve-affections often succeed to a slight injury, and he warns his pupils to be careful in treating the latter and perpetuating a purely nervous complaint. It will be apposite to the subject if I quote some paragraphs of his work.

"In a great number of instances, local hysterical symptoms appear to be connected with some accidental injury; generally a very slight one; and they are then especially liable to be misunderstood and mistaken for something very different from what they really are. For example, a woman is bled in the arm. She complains of pain extending down the forearm to the hand, up the arm to the axilla and shoulder or to the neck. You examine the cicatrix, and discover nothing unusual in it; but the patient flinches when it is touched. In another case, the patient has received a blow on the head; she subsequently complains of pain in it, and many other sensations referable to the seat of injury. In another case, a young woman pricks her finger, or perhaps the finger is merely pinched. Soon afterwards she complains of pain extending from the finger upwards along the hand and forearm. This probably is followed by a convulsive action of the muscles of the arm, or by a continued contraction of the flexor muscles on the anterior part of the arm, so that the forearm is kept permanently bent, at least while the patient is awake; for the spasm is generally relaxed during sleep."

"A young lady, eleven or twelve years of age, pricked the forefinger of her left hand with the point of a pair of scissors.

This was immediately followed by pain in the course of the median nerve, and, on the following day, the forearm was fixed by muscular contraction at a right angle with the arm. After a few days, all the muscles of the hand and forearm were affected with violent spasms, producing strange convulsive movements of the hand and forearm. These were attended with sickness and vomiting. By degrees the other limbs became affected, and it was impossible for the patient to walk or even to stand; sometimes the diaphragm was affected, so as almost to threaten suffocation. At other times the jaw was closed by a contraction of the masseter muscle, or she lay in a state of opisthotonos."

It is important, therefore, in the attempt to elucidate the hysterical state, to remember how frequently a shock, either bodily or mental, appears to be its determining cause. A very marked example occurred to me some years ago in the case of a girl who, on returning home one Sunday evening from church, was assaulted or frightened by a man in a lonely road. She arrived home in an hysterical condition, and, when I saw her on the following day, she lay in bed, like a statue, perfectly senseless and motionless. She never moved a muscle, and a needle could be thrust into any part of her skin without her perceiving it. This condition lasted some weeks, when she recovered. It is clear that this girl's brain had lost its power or ceased to work. She could exercise no volition, and all perception was gone. If we consider that her brain was functionless, as in sleep, we have no difficulty in understanding the case of hysterical hemianæsthesia; it is a similar condition with one-half only of the brain affected. My own opinion is that, in these cases, one hemisphere has come to a state of rest; for, if this be not admitted by those who believe in localised functions, they must admit that a considerable portion must have ceased to perform its function when we have regard to the multiplicity of the symptoms. Having myself a belief in the double action of the brain, I have no difficulty in understanding how consciousness still remains.

The question of injury, however, is very important to consider from a practical point of view; for it might lead us to confine a patient to her couch for many months, or even lead to the performance of an operation, as I shall presently show. In the two cases just mentioned, the blow was received on the back; but it might have occurred on the head, and then the question of its importance would be a still more difficult one. Thus I published, in a volume of Guy's Hospital Reports, the case of a young man who was brought out of the streets into the hospital for dead. He was found in a fit, and, as afterwards appeared, was half-starved; he had been long out of work, and

had had several blows on the head. After he rallied, we found that he was almost helpless on the right side; his arm was flexed and contracted; his leg extended; he had total hemianæsthesia and partial loss of the special senses; there was also a suspicious lump on his head. This condition gradually passed off in about a month. If the case had been that of a woman, we should at once have pronounced it one of hysteria; and this diagnosis I at last gave, believing that some shock unknown to us, physical or moral, had been sufficient to suddenly arrest the working of one side of the brain in this predisposed young man.

Showing how important the question of injury is in such a case, I will quote an instance from Guthrie's work on *Injuries to the Head*, in which he trephined with success, but which case I think my readers will agree with me in considering to be one only of hysteria. Possibly, if the trephine had been applied to the skin, the cure would have been equally effectual. I will, however, first quote a genuine case of injury reported by Guthrie, resembling many which have since been published in proof of the motor functions of certain convolutions of the brain, in order to show how closely it resembles Guthrie's second case, and other undoubted examples of hysteria.

Charles Murray, aged 33, of the Foot Guards, was wounded at Waterloo on June 18th, 1815, by a piece of shell which struck him on the superior part of the left parietal bone. He remained insensible about half an hour, and, on recovering, was found to be unable to move his right arm and right leg, which hung dead and had lost their feeling. He was admitted into Antwerp Hospital two days afterwards, and, when he had been there four days, he was trephined, and some pieces of bone removed. Immediately afterwards, he was able to move the limbs, and was sensible when they were touched. He gradually recovered, so as to be sent home, the only symptom being some weakness of the right hand. The pulsation was still visible at the bottom of the wound. He was subsequently discharged cured.

M. A. Farnham, aged 23, a stout healthy-looking girl, received a blow two years ago from a stone falling from a doorway, under which she was passing, which struck her upon the left side of the head, at a spot an inch anterior to the parietal prominence. The immediate effect of the blow was insensibility, followed by acute pain in the head, which ever since continued to mark the seat of injury. A week after the receipt of the blow, she began to lose the power of moving the right arm; there being, however, no loss of sensation or any disturbance of the cerebral functions. During the following twelve months, the symptoms remained unchanged, and she visited several

hospitals without relief. The paralysis then became more complete, and she was finally admitted into the Westminster Hospital. The arm and leg were then quite paralysed; the arm, which had been flaccid, was rigid; and vision and hearing were slightly affected. Mr. Guthrie removed a disc of bone from the exact point in the parietal region to which she referred the pain. The portion of bone showed no disease. An hour afterwards, she raised the paralysed arm, and was able to extend the fingers; the pain was relieved, the countenance was less dull, and sensation was returning. During the following three days, fever and rigors appeared, as if inflammation of the brain were supervening. When these symptoms had passed, the paralysis had completely disappeared, and the sight and hearing were regained. Mr. Guthrie adds: "She has since had some relapse of pain and uneasiness in the head; but is altogether a different person, although of a very hysterical temperament. The cicatrix on the head is firm, and she considers herself to have been cured by the operation, although I find it difficult to say in what manner it was effected, or why the removal of the bone, which was in a perfectly natural state, should have given relief."

In considering a case of this kind, we may at once place it in the category which Brodie framed, and believe that a blow on any part of the body might have been productive of the same symptoms, or conclude that the injury to the head might have, in a more decided manner, caused some commotion in the brain, especially as the paralysis was on the opposite side to the blow. At all events, such cases must be taken into consideration in discussing the question of localisation of symptoms in connection with injury to the surface. It would be a good study to compare Guthrie's two cases of the soldier and the girl, and draw the pathological and clinical differences between them. Quite apart from the question of the localisation of very definite forms of spasms and paralysis, with injury to particular convolutions, the general fact remains that hemiplegia is a result of an implication of the whole surface. In hysteria, I believe, the action of the hemisphere has ceased, but we see the same thing in the more definite lesion produced by inflammation. Although I had for a long time been aware of the fact that those dying of unilateral arachnitis had suffered from hemiplegia, I endeavoured to explain it on the supposition that the deeper ganglia might have been involved; but, after the very accurate observations of Mr. Jonathan Hutchinson on the effects of inflammation of the brain, I was forced to admit its truth, and at once assented to his doctrine, that an unilateral meningitis was productive of paralysis of the opposite side of the body. I consider myself indebted to this surgeon for my present knowledge on this subject, as I am not aware

that anyone else had previously made similar observations. The production of special paralysis in consequence of localised lesions is a further development of the subject, as shown by Jackson and Ferrier.

Although I am arguing in favour of direct shocks to the nervous system, be they physical or moral, as instrumental in the production of paralysis of various kinds, I am quite open to the conviction that these conditions may be perpetuated by purely mental operations, and may also be removed by methods which act solely on the mind. As a good example of this, I may here allude to the termination of a case of hemianæsthesia, published in the *Journal*, by Mr. Horrocks, House Physician of Guy's Hospital, a few months ago. This girl, having passed through the whole ordeal of metallotherapy as well as the orthodox treatment by galvanism and medicines, remained perfectly incorrigible, and left the hospital just as she entered. The punctiform appearance of her anæsthetic arm showed how perseveringly she had been "worked at," to use my clerk's expression. A few weeks ago, her mother requested me to take her in again, which I did, and I then determined to practise a totally different plan, viz., to leave her alone. I ordered her nothing, and systematically passed by her bed, as if her case were not worth the trouble of discussing whilst there were so many sick people about her. In fact, I neglected her with a purpose, when one day I found her sitting at the side of the bed, which she had hitherto kept, and she told me that she could walk a little, and had some feeling in her right side. I then encouraged her; said that she would now make rapid progress. This she did, and soon left the hospital perfectly recovered. It, therefore, certainly seemed that the treatment by a "masterly inactivity" had been more successful than all the orthodox and novel methods practised on the first occasion. This is simply the experience which every medical man must have had in all kinds of hysterical disorders, where treatment is often as positively bad as neglect is as positively good.

In looking upon a woman's nervous system, I often picture to myself an instrument something like a differential thermometer, having a glass globe full of air and a number of pipes leading from it containing fluid, which are constantly influenced by the pressure within. I see the fluid in the pipes unceasingly oscillating or passing up and down under the slightest influence on the bulb, and, in like manner, I see in my imagination a nervous fluid continually in motion, or may be in undulation, through the whole of the nerves of the body. I witness then, as in the girl I mentioned who received a shock to her brain, all undulation cease; in another, cease on one side of the body only. The fright or blow may stop the cur-

rent, and the limb is for all purposes dead; or it may have an opposite effect, and throw it into a state of jactitation. If these slight causes will so derange the nervous apparatus, it is not surprising that equally slight causes will set it in order again. The nerve-force seems to be constantly ebbing and flowing under the influence of the most trivial impulses. A fall on a medicine-bottle deprives a girl of movement and feeling on one side, as well as of the use of her special senses, and a sovereign tied on her leg will bring them all back again. I am often reminded of a remarkable watch belonging to a schoolfellow in my younger days, and which, in modern phraseology, was in a state of unstable equilibrium. The mere taking it from his pocket or looking at it would often arrest its movement, when a slight knock would send it on again. The works had never been good, or, being out of repair, the slightest jar would stop their action; and, what seemed remarkable, another blow would again start them into movement. So in these women, with their unstable nerves: a violent shock paralyses them, and another shock, be it galvanic or of a cry of alarm, will set them again in action. But not only a sudden effort of the will will set right the machinery, but the application of metals to the limbs or a trephine to the head.

I can, therefore, understand how a strong effort of the will will rouse the dormant powers; that is how the healthy hemisphere can send a stimulus to the inactive one; but this fact does not cover all the forms of cure which we see, nor does it afford an explanation of the production of the first symptoms. I do not see how a mere negation of the will can deprive a person of common sensation and the use of all her senses. As I before said, until the nature of nerve-force is better known, the explanation cannot be forthcoming.—*British Medical Journal*, Jan. 18, 1879, p. 71.

25.—INVOLUNTARY ROTATION OF THE HEAD CURED BY CONTINUED PRESSURE ON THE NECK.

Under the care of Dr. HEATON, Senior Physician to the Leeds General Infirmary.

Alice N., aged twenty-two, domestic servant, was admitted on September 27th, 1878. She had a fresh complexion, healthy appearance, rather sturdy frame. There was no history of previous neurotic symptoms, nor of any such conditions existing in her family. Four years ago, having been overworked after the death of her mother, she began to have tremors affecting the head and the right arm. For this she was treated for four months, in the Infirmary at Kendal, with cold baths and some

internal remedies; afterwards she went to the Convalescent Hospital at Southport, but without improvement. Subsequently, she was treated by Mr. Hartley, of Settle, who applied blisters on each side of the spine, after which the movements ceased for six months. Then the movements of the head began, as she was now affected, and they continued till the time of her admission. When she was in the upright position, the head continually rotated rapidly, with about one hundred to-and-fro movements per minute, in regular time. She was quite unable to restrain this movement, except by supporting the head with her hand; it was effected with considerable force, requiring firm pressure to keep the head at rest. When she lay down, the movement ceased. She had no pain, and her general health was good. Menstruation was regular. There was no unnatural movement of the arms, or of other parts of the body than the head at the present time. She was much wearied with the constant movement of the head, which made her neck and shoulders ache.

She was treated medicinally for several weeks, with conium, arsenic, and inunctions of belladonna liniment along the course of the sternomastoid muscles, but without any result. Some decayed teeth, which had formerly given trouble, were extracted, with no better success.

In examining this patient one day, Dr. Jacob, the resident physician, discovered that firm pressure with the finger over a spot a little below and anterior to the root of the ear, immediately behind the angle of the jaw, had the effect of completely arresting the movement. This effect was most perfectly and easily produced by pressure on the left side, but on either side the result could be obtained. This effect of pressure in arresting the movement was as prompt and complete as the stopping of a machine when disconnected from the motive force; and equally prompt and decided was the return of the movement when the pressure was taken off. Galvanic currents between the spine and the pressure-points had no effect upon the movements.

An apparatus was subsequently contrived, consisting of a curved steel band fitting on the back of the neck, and having at each end a small pad resting upon one of the pressure-points, and of which the pressure could be regulated by a screw. This she could wear comfortably, and it was quite hidden by a neckerchief. It had the effect, when worn, of completely arresting the rotation, and restoring to the girl the natural control over the movements of her head. She wore this instrument with much satisfaction for some weeks in the Infirmary. When it was removed the movements returned, but she thought they were gradually becoming weaker and less exten-

sive, as she continued its use. As she was in good health in other respects, she was dismissed to her home on November 30th, with instructions to appear and report herself at the Infirmary from time to time.

In the middle of January of this year, she presented herself to me at the Infirmary, looking well and cheerful, and possessing the natural control over the movements of her head. She had continued to wear the instrument, with a gradual mitigation of her complaint, until she finally was able to discard its use altogether. Naturally, she was much pleased at being relieved from the discomfort and annoyance of the ceaseless movement of her head, which had troubled her for more than three years. She said that she sometimes had twitchings in her eye and arm, showing some remaining tendency to abnormal mobility of the muscles. She retained the instrument for further use if required by any return of the complaint.

Remarks.—The curious pathological fact of the existence of points on the surface of the body, by pressure of which some morbid uncontrollable movement can be arrested, has been observed in various instances. One or more such points have been discovered on the surface of the same patient; sometimes these have been symmetrically placed, as in our case—sometimes dispersed irregularly. The point of efficient pressure sometimes overlies the course of the motor nerve of the affected muscles; thus, for example, some convulsive movements of the face have been arrested by pressure over a branch of the trigeminus nerve. In other instances, no such relation can be observed; and this must probably be the case where several pressure-points have been discovered, placed irregularly.

The pressure-points in our case would overlies branches of the cervical plexus, from some of which the muscles concerned in the rotatory movements derive their power. In such case, the suggestion that the pressure temporarily arrests the transmission of nervous vibrations reflected from the spinal centre to the affected muscles affords the most obvious explanation; though it is not equally applicable to some other cases, and though it leaves us quite uninformed as to the nature and the locality of the initial irritation. Morbid involuntary muscular contractions which are *tonic*, stiffening the affected part, are frequently the result of some direct irritation acting on the motor nerve in some part of its course between its spinal origin and the affected muscle; whereas *clonic* spasms, which alternate with relaxations and occasion movements of the part affected, are more frequently caused by reflex irritation; such movements being commonly convulsive and irregular. In ordinary cases of torticollis, for example, the head is habitually drawn to one side, by the constant undue contraction of the muscles on one side of the

neck; in some other cases, the morbid action is more convulsive, occasioning irregular jerking movements of the head which are involuntary and emotional; the steady rythmical uniformity of the horizontal semi-rotations of our patient are a more exceptional phenomenon.

Our object in the use of the instrument which I have described was, in the first place, to provide a means of *relief*, by arresting the movements so long as the instrument was applied. We were not very sanguine of effecting a permanent cure, and the pressure came so near to the course of the carotid artery, that it was feared that some inconvenience might arise from interference with the circulation in the brain. But no such inconvenience was experienced; and the instrument was worn so comfortably, and it so effectually abolished the unnatural movement of the head so long as it was in use, that it continued to be worn habitually by the patient. The very satisfactory result which has followed from its continued use, without the combination of any other means, furnishes an example of the curative effect of a persistent resistance to a morbid action. Here this has been effected, not by any forcible restraint of the moving organ or limb, but by the more subtle means of arresting the exciting influence, which in time has thus been, not arrested merely, but extinguished. The completeness of the present cure of a complaint which has persisted for upwards of three years, and which belongs to a class of disorders which are commonly very intractable, is a remarkable and satisfactory result. As to the permanency of the cure, we must leave the question to be settled by future observation.—*British Medical Journal*, Feb. 15, 1879, p. 228.

26.—ON CALABAR BEAN IN IDIOPATHIC TETANUS.

By Dr. REGINALD BLIGH READ, Sydney.

The few brief notes I now send on the value of large doses of Calabar bean in a case of idiopathic (?) tetanus in a lad, æt. $12\frac{1}{4}$, would I think have had much interest for my valued fellow-student and friend, poor Anstie. The only history I obtained was that of a fall the previous June or July; on 26th September a shivering fit whilst in school; on 28th a severe wetting and chill; on 30th a stiff neck; seen by me on 4th October, at 4 p.m. General tetanus supervened on the evening of the 7th October, but yielded readily to chloral hydrate given by the mouth and per anum, but which soon ceased to produce the desired effect. On the 10th at 4 p.m. he was given alcoholic extract of Calabar bean. As I was at the time overwhelmed with other urgent engagements, and the patient resided a long way from my house, I could not superintend the administra-

tion of the bean, and so I only gave 1 gr. of the extract every four hours. This proving satisfactory, the dose was increased to $1\frac{1}{2}$ grs. The rate every twenty-four hours was then rapidly increased to 23, 45, 60, 82, till it reached 90 grains, when the disease appeared to have reached its crisis, and three hourly doses of $3\frac{3}{4}$ grs. each were omitted, and at 8 p.m. of 25th October, $2\frac{1}{2}$ grs. were directed to be taken, and an extra dose of this quantity had to be given. Every attempt to reduce considerably the quantity of the bean taken was unsuccessful, and great difficulty was experienced in reducing the dose to 60 grs. daily, on 26th and 27th and part of 28th. The extract used up to the 18th was from Blackfriars, afterwards that prepared by Ransom of Hitchin, and was given by the mouth. The largest quantity actually taken in twenty-four hours was $83\frac{3}{4}$ grs., and I gave in twenty minutes on one occasion $7\frac{1}{2}$ grs.

In this case the perspiration I attribute entirely to the tetanic spasms, as invariably when brought under full doses (for him) of the bean *all* perspiration was checked. The temperature was also lowered; at 9 p.m. of the 26th, consequent on a violent return of all the symptoms, I gave the $7\frac{1}{2}$ grs. in twenty minutes. At 10 a.m. of that day the temperature in the rectum was $103\cdot2$, pulse 120. At 8 a.m. of the 27th, the temperature in the rectum was $99\cdot5$, pulse 94. The bean also checked the frothy mucus in the air-passages, the salivation it produced being quite distinct. Comfortable decubitus and easy sleep, from which he readily roused when I spoke to him, and the ability to take large quantities of nourishment, were also owing to it. Only when under the influence of very large doses could contraction of the pupil be obtained. No matter what the dose, he *never vomited*, though once or twice when first given he would say he felt sick. Rarely did he complain of pain in the bowels, though free evacuations were daily obtained. When the doses were first increased to $2\frac{1}{4}$ grs. every two hours, the bean produced an intoxicating stage with very rapid breathing, which could hardly at any time be called stertorous, but rather hysterical; a few intermissions, say five, of the rapid breathing would occur, and then the quick inspirations would be resumed: this would continue until the respirations slowed down to such an extent that the parents several times thought the lad was gone. This stage lasted five minutes. On altering the dose to 1 gr. an hour this effect disappeared, though I could always see that there were traces of it after every dose, but not very great even when the dose was $3\frac{3}{4}$ grs. every hour. Urine scanty and bowels freely opened throughout. As much nourishment given as possible. On one or two days he complained of toothache, so I removed a canine milk tooth, over which the new one was growing out; this relieved the neuralgic

pain. Intellect very clear, and he chimed in with any conversation going on, though whilst in the stage of intoxication he would be slightly delirious. The highest temperature in the rectum was on the 24th, when the thermometer marked 104·6, whilst pulse was 140. This day appeared to be the crisis of the disease. On October 11th, herpes labialis appeared, which disappeared on the 19th, and was followed on the 20th by an eruption on the back of solitary vesicles as large as a pea, evidently enormous sudamina, which spread to the chest, then neck, face, and head, then to the palms of the hands and soles of the feet, causing great itchiness. At first glance he looked like a small-pox patient. I consider them *herpetoid*, and to be the effect of the disease and not of the bean. From the time the general tetanus supervened I had the able assistance and advice of my friend Dr. Marshall of this city, who followed the course of the case with unflagging interest.

At 10.10 a.m. this morning, the 28th October, the sufferer expired with a single sigh, but cheerful and sensible to the last. The temperature in rectum an hour before death was 104, of the skin 102·1; pulse 118; respiration 40, accompanied by large mucous râles, which have shown themselves occasionally disappearing since the 23rd. No post-mortem allowed. Death from exhaustion occurred on the 29th day of tetanus, or 33rd day from the shiver. There seems much in this case to cause me to consider it as an intermediate one between tetanus and cerebro-spinal fever. When large doses of the bean were given I devoted all the time I could possibly spare to this case, otherwise would have sent you fuller notes, but being much worn out myself will send them at a later period, should you wish them.

The additions of the doses having been made up, I have just time to add them here.

			Grains.
10th October, 4. p.m. to midnight inclusive,			3
11	"	"	7
12	"	"	16 $\frac{1}{6}$
13	"	"	18 $\frac{1}{9}$
14	"	"	16 $\frac{9}{16}$
15	"	"	25 $\frac{5}{8}$
16	"	"	21 $\frac{1}{4}$
17	"	"	17 $\frac{1}{2}$
18	"	"	31 $\frac{7}{16}$
19	"	"	41 $\frac{9}{16}$
20	"	"	47 $\frac{1}{4}$
21st	"	"	56 $\frac{5}{16}$
22nd	"	"	66 $\frac{5}{8}$
23rd	"	"	75 $\frac{1}{16}$

24th October, 4 p.m. to midnight inclusive,	68 $\frac{3}{4}$
25 " " " "	60
26 " " " "	52 $\frac{1.5}{16}$
27 " " " "	65 $\frac{5}{8}$
28 " " 9.15 a.m. "	17 $\frac{1}{2}$
<hr/>	
Total	707 $\frac{4.17}{434}$

From 4.15 p.m. of October 23rd, to 4 p.m. of 24th, 83 $\frac{3}{4}$ grains taken.—*Practitioner*, March 1879, p. 192.

27.—ON THE DIAGNOSIS OF ALCOHOLIC COMA.

By Dr. WILLIAM MACEWEN, Surgeon and Lecturer on Clinical Surgery, Royal Infirmary, Casualty Surgeon, Glasgow.

Many persons have been pronounced to be suffering from alcoholic intoxication who, in reality, were affected by something more serious, such as concussion of the brain, fracture of the skull, opium poisoning, apoplexy, &c.

Dr. Richardson states that, in apoplexy, the temperature of the body is above, in drunkenness below, the natural standard of 98° of Fahrenheit's scale, and upon this statement he founds a distinguishing test.

There can be nothing more certain than a thermic test, as it supplies physical evidence, and is easily obtained. It has been well established that alcohol in excess reduces the animal temperature, and Richardson states that, as the fourth stage of alcoholism is approached, the decline in temperature becomes actually dangerous, and that in man the fall is often 2 $\frac{1}{2}$ ° to 3°. Though the general fact of the reduction of the temperature was quite familiar to me, yet my experience in alcoholic insensibility indicated considerable variation in the temperature in different individuals; so much so, that I feared it might invalidate the trustworthiness of the thermic test. In order, however, to place the matter beyond doubt, I resolved that I would record the temperature in the next fifty cases of alcoholic insensibility which came under my observation.

I have now obtained a record of over fifty cases of the insensible from alcohol, which, for tabular convenience, I have restricted to fifty. I think it right to state that I was assisted in recording these temperatures by my late house surgeon and friend, Dr. Pinkerton. These observations were rectal, and they were taken by instruments corrected and certified at Kew. The material, though largely composed of the poorer classes, was yet fairly representative of the various elements of society figuring in such a commercial centre as our own. The ages reached from eighteen months to sixty years: five being below twenty, nine between twenty and thirty, twenty between thirty

and forty, thirteen between forty and fifty, one at fifty-eight, and two at sixty years. Of these, three were females. The observations extended over a year, and were made from 7 p.m. till 1 a.m., but the majority were made about 12 midnight. The temperature of the surrounding medium—viz., the room in which they were seen, ranged from 60° to 70° F. Some were, however, brought, within half-an-hour of the thermic observation, from the external atmosphere to which they had been exposed for variable periods. The whole of the persons whose temperatures are here recorded were so insensible that they could not be roused. The temperatures ranged from 98°·2 to 93°·4.

Two	were recorded at	.	.	.	98°·2
Fourteen	„	from	.	.	97° to 97°·9.
Fifteen	„	„	.	.	96° to 96°·9.
Twelve	„	„	.	.	95° to 95°·9.
Five	„	„	.	.	94° to 94°·9.
Two	„	at	.	.	93°·4.

In this way eighty-two per cent. ranged from 95° to 97°·9, and fourteen per cent. from 93°·4 to 94°·9, while four per cent. were at 98°·2. This gives a range of temperature of five degrees for alcoholic coma.

It must be borne in mind that these are rectal temperatures, and that Wunderlich reckons from one half to nearly a degree higher in the rectum than in the axilla.

It will be observed that many of these temperatures are very low. Taking Richardson's statement, that the temperature in alcoholic coma is reduced from $2\frac{1}{2}^{\circ}$ to 3° below normal—(even leaving out of consideration the difference between the rectal and axillary temperatures), the lowest point ought to be 95°. Even this, according to former experience, would be reckoned a very low temperature, but the observations just recorded register five cases at 94°, and two at 93°·4, the latter being nearly two degrees lower. It need scarcely be added that these temperatures took me by surprise; so much so, that I was inclined to doubt their correctness, and to attribute the lowness to some peculiarity of the instrument or mode of observation. On narrowly scrutinising both these points I could discover neither flaw nor error; and the cases presenting a temperature of 94° (and some decimal points) became sufficiently numerous to permit of the fact being put beyond doubt. The first observation at 94° was several times repeated before it was accepted. In like manner, when 93°·4 was registered, I said to one of my pupils, Mr. John Middleton, who was present, that some mistake had been made. The instrument was therefore readjusted and again introduced—this time fully three inches into the rectum—and retained there for six minutes, and again

93°·4 was recorded. So that there can be no mistake about that fact. The person on whom this observation was made recovered, and so did another case, having a temperature of 93°·4; but the latter had to be carefully watched for twenty-four hours, in the infirmary. From these facts it may be concluded that the temperature in man, during a state of alcoholic coma, has a latitude of five degrees; ranging from what may be considered normal down to 93°·4. While alcoholic coma has a variable temperature, and, therefore, a thermic test cannot be absolute, yet, the greater majority of cases exhibit a marked depreciation in temperature. The question which must now be asked is, whether the insensible from other causes than alcohol can be distinguished by this thermic test? In order that other causes of insensibility might be differentiated they would require to have a temperature above 98°·2.

While carrying out those observations on alcoholic insensibility, I also made a note of the temperatures exhibited by several other causes of coma. Thus, the thermic state of persons suffering from *fracture of the skull* was noted, and while finding that the temperature was subject to very great fluctuation, it was seen, in several instances, that the temperature was much below the normal, in one case reaching as low as 94°·4 (vaginal temperature). Not only did the temperature vary in different cases, but it changed rapidly in the same individual within a short space of time. One instance gave a temperature of 97°·2 when first noticed; when the instrument was introduced two hours after, it indicated 100°, and within twelve hours from the first observation, it was 104°. Again, in two cases of *opium* poisoning, the temperature was much below the normal, the one indicating 97°, the other 96°·5, and I have no doubt but that still lower temperatures may be recorded in severe cases of opium poisoning. Then as regards *apoplexy*, it was found, in two personal observations, that the temperature of the one was below normal, the other about normal (99°·4). As these two isolated observations would be of little service, in the way of drawing conclusions, I deemed it expedient to consult my friend and colleague, Dr. Robertson, whose large experience in nervous affections placed him in a position to give an authoritative answer to this question. Dr. Robertson stated that, all his cases of apoplectic seizures showed an initial reduction of temperature, and that within two hours of the seizure, the fall, as tested in the rectum, had been from one to two degrees. Another authority, Bourneville, one of Charcot's former pupils, divides these cases of apoplexy, due to cerebral hemorrhage, into four categories, and for our present purpose it is enough to note that, in every one of these, there is an initial lowering of the temperature in apoplexy, and that it

may even sink as low as 96°. In some cases this lowering continued until death; in others it was followed by either a stationary period, and then an increase, or by a rapid increase above the normal, without a period of rest. From these it is gathered that in apoplexy there is an initial lowering of the temperature, sinking one or two degrees, and sometimes even reaching 96°.

Thus it is seen, that an insensible condition proceeding from alcohol, fracture of the skull, opium poisoning, and apoplexy, are all of them subject, at times, to exhibit low temperatures, and, therefore, the thermic test *cannot* be relied on as a means of differential diagnosis between these various conditions.

Coming now to personal experience, it is incumbent upon me to state that, at first, I found the discrimination between the various causes of insensibility to be, in certain cases, one of the most difficult tasks, and had the current teaching and text book literature been followed, many serious errors would have been made. Observations showed that there was considerable divergence between the generally recorded opinion and the facts regarding alcoholic coma, and this was considered the weak point in the differential diagnosis. If the diagnosis of alcoholic insensibility were once elucidated, other causes of insensibility would the more easily be relegated to their respective origins. And, at least, if the matter of alcoholic insensibility were eliminated, the case would then assume, to the examiner's mind, a more serious aspect, and one demanding careful scrutiny and supervision; consequently it would preclude the possibility of accident from non-attention arising from the apparent triviality of the case. It was, therefore, decided to examine closely the appearances of alcoholic coma.

Without occupying your time with details concerning this investigation, I will direct your attention at once to a point which I believe will facilitate the diagnosis of this question.

When first I had to deal with alcoholic coma, the text books invariably taught that the pupils in that state were dilated, whereas one case after another came under my observation, with contracted pupils, and as time went on, it became evident that this was the rule. Here, then, was a discrepancy between the generally received opinion and my observations, and the question as to the origin of this difference naturally presented itself. Either this must have been an error on the part of the observers, including myself, or the facts themselves must have varied. With regard to former observers, some, at least, of them could be relied on, and it was extremely unlikely that all of them had committed an error on such a simple point. But here were my own data, abundantly demonstrating the opposite state of the pupil. Was it possible that this divergence

depended on some peculiarity in the alcohol consumed? This was extremely improbable, owing to the wide area from which the material for observation was supplied; the spirit used must have been of very varied kind and quality, manufactured by many makers, both home and foreign, so that it might be considered fairly representative of the kinds supplied in a great city. It could not, therefore, have been due to any peculiar kind of alcohol. While this question remained undecided, the cases showing contracted pupils still accumulated; but at last I found a case of undoubted alcoholic coma with a dilated pupil. Having satisfied myself as to that, I set to work to find out the cause of this peculiarity, making minute inquiries about his antecedents, the kind of alcohol he had consumed, &c.; all of which inquiries proved futile in the way of shedding light on the subject. Half-an-hour afterwards, on returning to the patient, accompanied by a medical friend to whom I intended showing the first case I had met with of alcoholic coma having dilated pupils, to my astonishment, on raising the eyelids, the pupils were then found contracted just as I had always found them in such cases. While endeavouring to elucidate the cause of this change, it occurred to me that the only alteration in his external condition, between this visit and the former, was the fact that on the first occasion he had been moved, having just been lifted on to a stretcher, carried up a stair, and then laid on a bench. Thinking, therefore, that movement might have produced this change on the pupils, it was determined to test it. When about to carry out this project, he accidentally received a severe shake, when his iris immediately gave a series of waves, ending in a semi-dilated pupil, and after remaining in this state for some time, they slowly began to contract again, and in the space of twenty minutes they had assumed their former state of contraction. The shake was repeated experimentally, and though the patient was in no way roused thereby, yet his pupils underwent the dilatation and subsequent contraction in about the same time. Extended experience showed that the contracted pupil of alcoholic coma dilated on the application of external stimulus, such as by pulling the beard or hair, and that, as a rule, within half-an-hour, or less, the pupil again assumed its former contraction. But if the comatose state were about to pass off, the pupil, after it had been dilated by external stimulation, might then either only contract a little, but not to its former state, or it might remain in a semi-dilated condition. Here, then, is the probable solution of the discrepancy as to the state of the pupil. Former observers probably made their notes either at a period prior to coma, when the person was still attempting to move about, or if they were comatose at the time, efforts were probably being

made to rouse them by shaking or other means, when the pupil would then present its dilatation. On the other hand, they were first seen by me while they were lying undisturbed in a deeply insensible state, and as they remained in that state the pupils were contracted. The pupil is contracted when the person is left undisturbed, and it is dilated when an attempt is made to rouse him. It is this order of succession which makes it characteristic, I may almost say, pathognomonic of alcoholic coma. Given an insensible person, who has lain undisturbed from ten to thirty minutes, who then presents contracted pupils, which dilate on the application of external stimulation (without in any way rousing the patient), and which, if the person is left undisturbed, begin to contract again within a short time, and I know of no other state to which this applies than alcoholic coma. It seems as if on the application of external stimulation, though the personal consciousness refuses to return, that the pupil half wakes up and then slowly drops to sleep again. It is nearer the state of the pupil in natural sleep than under any morbid condition, only, the pupils are more sluggish in their movements, and respond feebly to light and shade. The very process of shaking the person, necessary in this test, would at once rouse a sleeper, and so distinguish between sleep and coma, if this indeed were at all necessary.

In order to put this phenomenon of the alcoholic pupil to the test, it was resolved to take a note of fifty consecutive cases, and see whether this sign would remain a guide for alcoholic coma.

Of these fifty cases, forty-nine had contracted pupils. Forty-seven of these dilated on the application of external stimulation, and afterwards underwent contraction, either partially or quite to their former condition. In many instances this contraction took place in five minutes; in the majority from ten to twenty minutes elapsed before it occurred, while a few took as much as half-an-hour before the pupils had reached their former stage of contraction. One or two, though they did contract a little, yet did not pass the stage of medium, and in these cases the persons recovered from their insensibility very soon after. Now, two of the forty-nine cases that had contracted pupils were somewhat peculiar. In the one there was absolutely no dilatation on the application of stimulus; in the other, the pupil did slightly dilate, but only to a very limited extent, and that with great hesitancy, resembling, in this particular, the pupils of some cases of fracture of the skull. These two patients were watched pending the solution of these peculiarities. After they had recovered, the iris was seen to have been affected by former disease, in the one case fixing the pupil completely, in the other partially so.

The single case out of the fifty that had dilated pupils was

found to have fallen a short time before coming under my observation, and this had, no doubt, sufficed to dilate the pupils, as they were found to have undergone contraction after the lapse of twenty minutes.

From these it is seen that ninety-four per cent. of those suffering from alcoholic coma had contracted pupils, which dilated on the application of external stimulation; and the majority of these, after various intervals recontracted. Four per cent. had contracted pupils, which, owing to physical peculiarity, would not dilate; while two per cent. were dilated, owing to external stimulation, of an accidental kind, having been applied shortly before the observation was taken. But this last item may be deducted from the list of exceptions, as it, in reality, did not present a departure from the rule; the peculiarity being, that the observation began in the middle, as it were, when the pupil was found dilated. Therefore, it may be said that ninety-six per cent. of the cases responded to this test.

Now, it is important to note the precise conditions under which these observations took place; otherwise, they may lead into error. As a rule, the subjects of observation here spoken of had remained in the recumbent position for, at least, half-an-hour prior to the pupil test being put into practice, though some few cases were seen after a much shorter time, and yet exhibited a quite characteristic pupil. From the mere statement regarding the dilatation of the pupil, under external stimulation, it may be inferred, that one condition necessary to the contraction of the pupil is, that the person be allowed to remain at rest for some time,—the exact time necessary I am not at present able to say, it varies according to the depth of the coma; but I believe half-an-hour to be ample, and I have seen it at the end of five minutes. The pupils were always contracted to such an extent as to enable any surgeon to pronounce them indubitably so. Many times they have been pin head, and they have been more than once mistaken by surgeons for those of opium poisoning. In the case of a child aged eighteen months, the pupils were so minute as to be barely discernible. I am speaking here of the comatose condition, but it may be noted by the way, that, in the lesser stages of alcoholic insensibility, they are less contracted and have a greater tendency to dilate on slighter external stimulus. Among these fifty cases I included only those persons who were absolutely comatose, and who, for the time, could not be roused.

Under these circumstances, the pupil test affords very ample means of identifying alcoholic coma, and any errors that might be made, owing to finding a fixed contracted pupil, or even a fixed dilated one, would only be a slip on the safe side, on the side of extra caution, and not one that would lead to mischief.—*Glasgow Medical Journal*, Jan. 1879, p. 1.

28.—ON THE TREATMENT OF DELIRIUM TREMENS.

By Dr. GEORGE W. BALFOUR, Physician to the Edinburgh Royal Infirmary.

The introduction of the bromide of potassium into medical practice, and its recognition as a safe and reliable nervine sedative, ushered in a new era in the treatment of delirium tremens. Instead of waiting and watching through a tedious convalescence, during which the patient ran various risks of death, from twelve to twenty-four hours' treatment was enough to induce a sufficient amount of refreshing sleep to restore the patient to a rational condition and speedy convalescence. From a pretty considerable experience of this treatment, I can say that it only failed in very exceptional conditions, and in some of these failures the cure was completed by the subcutaneous injection of morphia; while the few cases in which death occurred were in patients exhausted by primary disease or maltreatment, or affected by severe epileptic convulsions—a form of disease usually amenable to the bromide of potassium, but which, as a precursor of delirium tremens, too often proved intractable and fatal. Only in the very rarest cases was it found necessary to conjoin this treatment with the administration of stimulants, and these cases were always tedious and most unsatisfactory. On the other hand, several cases treated outside, ineffectually, by the bromide of potassium accompanied with stimulants, were at once cured by the same remedy *minus* the stimulants. One of the most remarkable of these cases was sent in by a medical man, who seemed to think the case a very serious one. He anxiously inquired if I thought it possible that the patient, should he survive, would be well enough to be removed at the end of a fortnight, as his passage to America was taken, and his friends were anxious to get him away. He smiled incredulously when I told him if he called back next day he would find his patient either well or asleep, and fit to be removed at the end of a week. The result was, however, as predicted, and my friend is now a firm believer in the air of Ward 10, though still sceptical as to the virtue of the bromide of potassium. The dose of the bromide was, however, large—half a drachm or more; and required to be frequently administered—every hour—and that often till so much as ten or more doses were given before it took effect. In such cases this frequent repetition of the dose was always irksome and often troublesome, and the recent introduction of the hydrate of chloral was therefore welcomed as a possibly useful substitute. The first case of delirium tremens submitted to it was one of a fortnight's duration and of maniacal ferocity. He had had the bromide of potassium at home, and was sent in because it was found impossible to manage him, and his case was looked upon as

most dangerous. Two doses of hydrate of chloral, of thirty grains each, with an interval of an hour between each dose, sufficed to induce refreshing sleep, from which the raving maniac awoke a rational man, requiring no further special treatment. Similar success has attended the chloral treatment in all the cases of delirium tremens which have been admitted to Ward 10 of late. In several severely maniacal cases a dose of forty-five grains has been administered, with a result equally gratifying and surprising. In a case of puerperal mania one such dose sufficed to restore reason to the patient, at least so far as quietness and docility were concerned, though it completely failed to make her believe that she ever had given birth to a child.

So far, therefore, as our present experience is concerned, we seem to possess in hydrate of chloral a remedy which in all such cases, from the slightest to the most severe, acts rapidly, safely, and efficaciously—*cito, tuto, et jucunde*—and which seems to deprive indulgence in drink of all its horrors, and nearly all its dangers. Unquestionably fatal cases must occasionally occur under this as well as under other modes of treatment, but the number of them must be much decreased, because, from the rapidity with which a cure is brought about, many dangerous risks are averted. Thus, we avoid all the risks arising from a long continuance of maniacal excitement, or from a suicidal state of mind, all risk from the exhaustion following persistent sleeplessness, or defective nutrition, the result of long-continued insufficiency of food, &c. The risks the patient actually runs are not now, as formerly, connected with the treatment, but with his previous state of health. Thus, if he has a fatty heart, or has been exhausted by long-continued debauchery, or if he is from any cause an epileptic, he may die suddenly during the attack. But if he is otherwise healthy, he is sure of a safe and speedy convalescence.

So much for the treatment of delirium tremens. The fallacy to which I promised to direct your attention is this—that delirium tremens does not arise from drinking, but from ceasing to drink. In regard to this matter I myself have no doubt, and my confidence is derived from two sources:—First, I have found that so long as you permit your patient to obtain drink, just so long will his disease prove obstinate and intractable to treatment; while when you continue the treatment, minus the drink, the cure is rapidly obtained. Second, by stopping a man in his drinking by means of an antimonial emetic, you may often save him from an impending attack of delirium tremens, but you will never bring one on. My experience has always been that the longer the drinking was carried on the more severe the disease, and that by stopping the drink early we could always ensure a milder and safer disease.—*Lancet*, Feb. 1, 1879, p. 147.

29.—ON THE USE OF TINCTURE OF CAPSICUM IN EXTREME CASES OF DELIRIUM TREMENS.

By Dr. BINGHAM CROWTHER, House Surgeon to the General Hospital, Hobart Town.

The two cases I am about to mention will prove that we have in tincture of capsicum a most valuable remedy in dangerous cases of delirium tremens, and its action in the second case (complicated with pneumonia) points to a new line of treatment for that affection, for pneumonia occurs in this country chiefly in individuals who have been subjected to lowering agents (hard work, cold, wet, &c.) It must be owing to its stimulating and derivative effects that capsicum is of value both in the delirium tremens cases and in the pneumonic complication. It will be noted that capsicum acted in many ways in common in both the cases I am about to mention. First, the rapidity of its action, reducing all the tremor and agitation (in a few hours), followed by a calm and prolonged sleep. Second, the skin, from being clammy, is soon restored to its natural warmth, and perspires freely. Third, the pulse, which before its use is 140 to 150, rapidly falls to 100, and instead of being small and compressible, regains its fulness and volume. Fourth, both the kidneys and bowels act freely, large quantities of urine being passed frequently, and the alvine evacuations in large quantities (the bowels being continually relaxed whilst taking the capsicum). It must be to its derivative action that the brain so rapidly becomes tranquil. All these actions were well marked in the cases I am about to relate, and they seem, both from their therapeutical and physiological points of view, to be typical of the known action of capsicum.

Case 1.—G. E., aged forty-eight, admitted April 3rd, 1878. Is tall, fair, and well built, and for several nights has been without sleep. History of being a heavy drinker, chiefly of spirits. All the known ordinary remedies had been tried by his surgeon prior to admittance; the case becoming extreme necessitated his removal here. (The same patient was under treatment for three days in January, 1878, in a similar condition. Bromide of potassium, ten grains every four hours for two days, failed to relieve; and tincture of digitalis, in three doses of three drachms every four hours, produced a sound sleep and rapid recovery.) On admission he is constantly moving about, talking all sorts of rubbish, giving orders, &c., with muscular tremor. He answers some questions rationally when spoken to. Pulse 140, weak and compressible. Tincture of digitalis ordered in three doses of three drachms each, every four hours at night.—April 4th: No relief whatever; is in a similar condition to yesterday. Pulse 150, very weak and com-

pressible. No sleep.—5th: In the same condition, but worse, so weak he cannot stand, and seems in great danger of death from asthenia. Ordered tincture of hyoscyamus combined with bromide of potassium, but no relief taking place in twelve hours (he had taken four doses of the latter mixture), half a drachm of tincture of capsicum in half an ounce of water was given every three hours.—6th: Has slept since 4 a.m., and now (10.30 a.m.) is still resting. Pulse 110, full; respiration 24, easy; bowels opened freely through the night; the urine has been passed frequently and in large quantity.—8th: Continues to improve; frequent micturition; bowels open two or three times daily.—10th: Discharged well. Nearly all tremor had left. Sleeps well during both night and day. Has a firm, full pulse, and is very grateful for his rapid recovery. Diet, strong slops, with a pint of double beef-tea, porter, and other nourishing food.

Case 2.—Robert C., aged 36, admitted June 23rd, 1878. Patient is tall, dark, and well built; has been employed as drayman for seven years. Has been exposed to changes of weather, and enjoyed good health until six weeks ago, when he got wet. Pains all over body followed. Has been accustomed to take large draughts of beer, especially when heated, also at other times, more so lately. Four days before admittance called in a doctor, who found him suffering from delirium tremens, and ordered his removal here. On admission patient had marked delirium tremens, with hallucinations (driving horses, &c.) Breathing natural. Pulse 130, soft compressible. Answers questions in a jerking, tremulous way, not very distinctly. Ten grains of bromide of potassium ordered every four hours.—June 24th: Worse; is constantly moving about, although put to bed several times. No sleep since admittance. Pulse 140, very compressible; skin clammy; bowels have not acted yet. Has a dry cough. Condition one of danger. A mixture of tincture of capsicum with brandy and eggs was given every four hours. After taking first dose he fell into a calm sleep, and remained in that condition until afternoon.—25th: His cough is worse; also short breathing and rusty sputa attracted attention, and on examination he was found to have crepitation over whole of right lung. Respiration 45; pulse 140. (Pneumonia.) Passed much urine, and bowels have twice been opened during the evening. Skin warm, freely perspiring. Delirium abated. Transfer in the afternoon from the cells to general wards.—26th: Bronchial breathing and bronchophony over whole of right lung, and increased tactile vibration.—27th: Is worse. Pulse 140; respiration 50. Delirium again returning. Both urine and motions have passed freely daily. Skin very hot and perspiring.—28th: Is violently delirious, although

quiet when spoken to. Had an attendant all last night, and to-day was obliged to be again removed to a special ward. Life despaired of. Pulse 120; respiration 45. His capsicum mixture (which had been taken regularly) was now halved, and taken every six hours.—29th: Delirium subsided. Patient feels comfortable, and has much improved. Crepitation in lower half of right chest, rest of chest same as on 25th. Pulse full, 110. Takes his food and sleeps well. Alvine and urinary secretions continue free.—30th: Improvement continues. Respiration 36; pulse 100.—July 4th: His right chest has gradually resolved itself into a healthy condition, no abnormal sounds being heard. Is rapidly convalescing. Pulse 85; respiration 20. The diet has been nourishing; slops in a concentrated form, with two pints of ale (which latter was changed on the 28th for porter). Small doses of spirit of nitrous ether and carbonate of ammonia were combined with the mixture during the pneumonia.—*Lancet*, Jan. 25, 1879, p. 118.

30.—A CASE IN WHICH THE HYPODERMIC INJECTION OF MORPHIA WAS SUDDENLY DISCONTINUED AFTER ITS USE DAILY IN LARGE DOSES FOR SEVEN YEARS.

By Dr. JAMES BRAITHWAITE, Lecturer on Diseases of Women and Children at the Leeds School of Medicine, Surgeon to the Leeds Hospital for Women and Children.

The following case I believe to be unique, for I am not aware that anything of the kind is on record, and indeed the resolution necessary to carry out such an experiment can rarely be met with.

Seven years ago I was attending a young married lady on account of a painful affection of the spine. The nature of this was doubtful, but was at the time believed to be inflammatory by Sir James Simpson, Mr. William Hey, and my father, all of whom saw the case. I one day, unfortunately, as the event proved, injected her with morphia hypodermically, and at her request occasionally repeated the dose when the pain was more than usually severe. The patient herself began to use the morphia when from home at a watering-place, and the habit soon became confirmed, and the doses largely increased in strength. The dose she ultimately reached was fourteen grains a day. No bad effect of any kind was produced; she pursued her daily duties, and entered into society, but she was never fit for anything in the morning until she had been injected, and always kept quiet till it was time for her dose. If she had anything extra to do she had an extra injection, and this seemed to impart life and energy. During the four years the morphia was thus used she was occasionally ill, and for everything a little extra

morphia was a certain panacea, even for the flooding attending several miscarriages. These were got over with comparatively little loss of blood, and from this I received a hint which I have found of some use in treating abortions, and also in hæmoptysis, which form of hemorrhage [is rapidly and certainly checked by a dose of morphia hypodermically. An increasing difficulty in finding suitable places for injection, owing to the body becoming thinner, led to a strong desire to be free from the tyranny of the habit. The patient was a lady of great force of will, and it was all needed in the struggle. She gradually diminished the dose, till in two years it had been reduced to three quarters of a grain a day. If the diminution was too great, diarrhoea immediately resulted, and the patient could gauge the diminution which the system would tolerate by this symptom. My belief is that if nothing had occurred the morphia would have been got rid of by this gradual process, though very slowly, and with great difficulty. At this period, however, she became pregnant, and any further diminution was found to be impossible.

When six months advanced in pregnancy, in consequence of an injection, a large abscess formed in the lower and inner part of the left thigh, and from this the femoral vein could be traced upwards as a hard cord. This proved so serious, that I asked my friend Mr. Jessop to see the case with me, and I feel that to his unceasing kindness and skill much of the credit of the result is due. The abscess was opened antiseptically, and with great care did well, but erysipelas spread from it as a centre up the thigh and round both hips and the sacral region. The morphia had to be rapidly increased in strength to save her life and also to check the erysipelas, which it appeared to do in a wonderful way, for every fresh recurrence of erysipelas was checked at once by a further increase of the morphia.

She was confined on Feb. 6th, 1877, the erysipelas having barely disappeared. The amount of morphia then taken daily was eight grains. Just a fortnight after her confinement, at her own urgent request and with the concurrence of Mr. Jessop, it was decided that the morphia should be discontinued at once and entirely. She was injected for the last time on the morning of Feb. 20th. We had no facts as to other cases to guide us, except a short note of a somewhat similar case published in an old number of the Edinburgh Medical Journal, though not with reference to the use of morphia hypodermically. This, however, as it turned out, led us considerably to under-estimate the seriousness of the illness which would follow. Constant, and indeed incessant, vomiting and purging came on next day. The vomiting occurred about every ten minutes, and consisted of a mucoid fluid tinged with bile. The alvine evacuations were similar in

appearance, but darker and more tenacious. On the second day the vomiting and purging still continued, as they had done all night, the stomach rejecting everything, and even a nutrient enema by the bowel coming away at once. Ice, ice and champagne, milk, and lime-water, in very small quantities, were tried in vain. On the fifth morning the stomach retained for a short time a morsel of chicken, with a particle of bread, and about a dessert-spoonful of beer, but this was rejected in half an hour, and appeared to have aggravated the sickness. The stomach retained no food for more than a few minutes, till on the ninth day her life appeared in great danger, so great that I had the syringe ready charged, and urged her to let me inject her to save her life. She, however, was firm in her resolution, and said she would rather die than again become the slave of the morphia. The next day (the tenth) the stomach retained food (a morsel of fish) for a few hours, and after this the improvement was gradual, but certain. The diarrhœa, however, continued more or less, and the stomach would only bear food on one condition—that a very long interval should elapse between each meal. I believe the case would have ended here, but through some over-exertion the erysipelas returned on April 16th, and rapidly spread. This seemed to cause a renewal of all the old sickness and diarrhœa, and these lasted continuously for ten days more distressingly than before, and accompanied with such burning of the throat and mouth as to oblige her to have a wet sponge laid on her lips. After the tenth day improvement gradually, but much more slowly than on the previous occasion, set in; but up to this date (December, 1878) the diarrhœa still remains, and on the least over-exertion becomes aggravated. My impression is that it might have been got rid of long before this by absolute quiet and rest, but unfortunately I have never been able to enforce this, and the diarrhœa has now become so chronic that rest has very little effect upon it. There can be no question, however, that every fresh exertion causes an exacerbation of it.

I conclude from this case that a very gradual diminution of the morphia injected is preferable to a sudden discontinuance of it, and seeing that in two years she had got down from fourteen grains a day to three-quarters of a grain, a gradual diminution is proved perfectly practicable. It is, however, the opinion of this lady that there is a greater totality of suffering, but spread, of course, over a longer period of time. If, however, from any reason (as formation of abscesses), it is discontinued suddenly, the symptoms resulting will be incessant vomiting and diarrhœa. The vomiting will subside in a variable period, and the sooner, the less the stomach is teased with anything. The diarrhœa, however, will continue for a long period, and is

best treated by absolute bodily repose. Neither astringents nor enemata of any kind have any effect in checking it; rather the contrary. An enema of thirty grains of chloral hydrate in an ounce of tepid water at bedtime must be excepted, as this was retained, and proved of service on several occasions.

Against the plan of gradual diminution, however, it should be observed that the bad effect on the general health during the two years mentioned was very marked, the patient being rendered more or less an invalid. The opinion of the patient herself is very decided in favour of sudden discontinuance, and, as she is a woman of great ability and intelligence, her opinion is not to be disregarded. I cannot, however, agree with her, as I saw her vomiting every five or ten minutes for several days, both immediately after the discontinuance, and on two subsequent occasions when relapses occurred owing to some exertion, and the diarrhoea continues to the present time, nearly two years from its commencement, and is sometimes very exhausting. It is interesting to note that the large doses of morphia had no effect whatever upon the child, which was lively and active in the womb, and is, and always has been, perfectly strong and healthy.—*Lancet*, Dec. 21, 1878, p. 874.

DISEASES OF THE ORGANS OF CIRCULATION.

31.—ON THE NEUROSAL AND REFLEX AFFECTIONS OF THE HEART.

By Dr. J. MILNER FOTHERGILL, Assistant-Physician to the City of London Hospital for Diseases of the Chest, Victoria Park, and to the West London Hospital.

It has been stated by more than one writer on the diseases of the heart that when a patient comes into the physician's consulting room complaining of his or her heart, usually there is no cardiac disease; that is, unless previously told by some other medical man that there is heart mischief present. My own experience is quite in unison with this expression of opinion. The persons who complain of the heart being a source of disturbance have usually some neurosal affection of the heart; at other times it is palpitation due to some other cause which leads them to complain. The patient who has got organic disease usually complains of some outcome thereof, as shortness of breath on exertion. Often disease in the valves of the heart is discovered by the usual examination of all patients, and has existed some time unsuspected. But when the nervous mechanism of the heart is disturbed, then the patient is usually acutely conscious that something is wrong, and not rarely becomes much alarmed thereby; alarmed out of all proportion to the seriousness, or

rather absence of seriousness, of the case. Perhaps of all patients who have neurosal affections of the heart, medical men are the most unsatisfactory patients who present themselves to a physician. Unfortunately for their peace of mind, they are but too familiar with the insidious approach of some forms of heart disease, and therefore cannot dismiss from their minds their fears and apprehensions as to the interpretation of their symptoms. The ordinary patient is only too glad to take the physician's word for it that there is no disease of the heart—that it is a mere disturbance—and rejoices thereat; for it is a characteristic of humanity that it readily believes what it wishes to believe. But the unfortunate doctor himself is not so well satisfied, and thinks the subject over and over by the light of possibilities, until all his comfort is gone, and he has worked all his dreads and doubts into full action again. The consequence is, he is no better for the consultation for longer than a couple of weeks or so, but relapses into his old position; he is no better, and the physician achieves no credit. Such, at least, has been too much my experience, though not invariably the case.

The neurosal affections of the heart are of various kinds, and need some study for their full comprehension. There are those which arise in the cardiac ganglia themselves, and those which are of cerebral origin, and again those which are of reflex origin. Palpitation, irregularity (a disturbance of rhythm), and intermittency (a distinct halt), are the three forms of abnormal action. The heart is disturbed, too, by the emotions in a manner which puts emotional disturbance outside any single one of these divisions, and places it partly in all. We know that emotion will affect the circulation very markedly. In joyous emotion we have an excited action of the heart with full bloodvessels and warm extremities. On the other hand, when in dread or in anxiety the arterioles are contracted and the extremities are cold, while there is a rise of arterial tension producing a decided increase in the renal secretion, as is well known to those who are undergoing their examinations and are nervous as to the result. The frequent retirement of candidates for examination in order to empty the bladder has given rise to a peculiar and well-known expression familiar to all, but one which cannot very well be introduced here. This condition of the vascular system is identical with that which exists in hysterical attacks, when the arteries are contracted and the renal secretion is active. This alteration of the bloodvessels by emotion has not escaped the keen observation of our writers. Thus Charlotte Brontë makes Mr. Rochester feel Jane Eyre's pulse when he wishes to make sure that she will not flinch or turn sick at the sight of blood. “‘Just give me your hand,’ he said ;

‘it will not do to risk a fainting fit.’ I put my fingers into his. ‘Warm and steady,’ was the remark.” There was no palsying terror there. The circulation was free and undisturbed.

When the peripheral vessels are dilated, as is the case when the extremities are warm, the blood-pressure in the arteries is low, and the heart is not disturbed. On the other hand, when terror leads to contraction of the peripheral vessels—probably from the ordinary inhibition of the vasomotor centre being withdrawn—then there is a rise in the arterial tension, the ventricle has more obstruction to overcome, the internal pressure on the heart-walls is increased, and then palpitation follows. The beating heart of dread is thus probably rather a secondary condition, consequent upon changes in the blood-pressure in the arteries, than a primary emotional disturbance of the cardiac centres.

The blush, the *Schamroth* of the German, is also an emotional disturbance of the vascular system, and extends over the surface generally. It is not confined to the face merely, though on account of the great vascularity of the face it is most pronounced there, as well as being most visible there, the rest of the body being covered with clothes. It extends over the body, and Dieffenbach found a blush to extend over the nates and thighs of a female patient whose genitalia he was examining. When the blood-pressure in the arteries is suddenly lowered, the rhythm of the heart is momentarily disturbed; and the blush in girls is usually accompanied by excited action of the heart. In these emotional conditions the sympathetic nerve connexions of the heart are involved.

But there are other conditions where we must suppose the vagi to be the means by which the cardiac disturbance is induced. We know that the vagi exert a controlling or inhibitory action over the cardiac ganglia: that irritation of the vagi slows the heart’s action; indeed, if the irritation be sufficiently pronounced, the heart’s action can be completely arrested. A voluntary control over the heart can thus be effected by persons who possess the requisite strength of will. This is seen in the steady hand of the practised rifleshooter. If he could not control his emotional disturbance of the vascular system, the shock of the bounding pulse would shake his left arm and disturb his aim. This quality is popularly called “coolness.” So great is the voluntary control that can be exercised over the heart, that some persons can actually arrest the heart by an effort of will. Such was the case in the well-known Lieut.-Colonel Townsend, who did not, as is often said, kill himself by the exercise of this power. A well-known physiologist in this country can do the same thing to a surprising extent, but he does not practise the power unduly. Disturbance of the rhythm of the heart is a

well-known consequence of some forms of cerebral disease, as hydrocephalus, and is of bad prognostic omen. The heart's action is rendered irregular and unrhythmical in cases of compression of the brain, and inflammation of it (Marshall Hall). A curious instance of disturbance of the rhythm of the heart came under my notice lately in the person of a medical student. He was a tall, stalwart young man of good family history, but whose heart had acquired the trick of beating somewhat irregularly. While examining him one day I asked him a question, my ear being still on the stethoscope; he had to think a moment, and in doing so his heart's action became slowed and intermitted. This was often repeated on other visits; and whenever a question was asked involving thought for its answer, the disturbance of the rhythm of the heart was observed: questions not requiring thought for their answer did not produce this cardiac irregularity.

Disturbance of the heart is also produced by pressure on the vagus. The famous Czermak of Prague, the introducer of the laryngoscope, had a tumour in the neck, over which lay the vagus. On pressing the vagus upon this tumour the heart could be slowed and arrested. A well-known case is that related by Romberg in his work on Diseases of the Nervous System (vol. ii.). It occurred in Heine's wards in Vienna. Here the heart stood still for a distinct interval, the man during the time of the halt suffering intense agony. A tumour involving the vagus was diagnosed by Heine and Skoda, and found by Rokitsansky on the necropsy.

Neurosals angina pectoris is an affection of the nervous mechanism of the heart; but a more exact definition is not yet possible. In my own experience it has chiefly occurred in women at or near the menopause. In pronounced cases the agony induced is very great. True neurosal angina must be discriminated from that slight form of genuine angina found in gouty persons, where the heart is temporarily embarrassed by a sudden contraction of the arterioles raising the blood-pressure in the heart, and so rendering it more difficult for the ventricle to contract and overcome the obstruction so offered.

The usual symptom of neurosal disturbance in the heart is palpitation. It is found so associated most commonly in women. Here the heart is affected by paroxysms at intervals, the rhythm being normal during the interval; or there is a more or less persistent excited action of the heart. In some cases the action of the heart is excited, and it thumps against the chest-walls in a manner which is very distressing to patients, interfering with their work and causing them much annoyance, as preventing them from falling off asleep, or awakening them suddenly and alarming them. Of course, in such a persisting

condition there are paroxysms which are very disturbing. Such a heart might be designated "a badly behaved heart," for its behaviour is very bad indeed. It is a troublesome condition to treat, and requires time for the treatment to be successful. A well-marked instance of such a case is to be found in one of the out-patients of this hospital, a girl, whose mother also has a heart of like character. The mother has become practically cured for the time by a course of quinine and hydrobromic acid; but the daughter's case is a more troublesome one than the mother's. Another case, a Jewess, a tailoress, was a patient at Victoria Park Hospital. Being also anæmic, bromide of potassium with iron was prescribed for her. After several weeks, during which no relief even was afforded by the treatment, improvement suddenly set in, and her heart once more beat like other people's hearts. In these cases it seems that the heart is swung in a delicate nerve balance, so that it is readily disturbed by what would cause no perturbation in other hearts. This must be borne in mind in neurosal affections of the heart; just as we know that with some persons the liver is easily disturbed, in others the stomach, and in others again the brain, so there are people who possess hearts that are easily perturbed.

Palpitation is a phenomenon about which it is very desirable to have clear ideas. And in order to have clear ideas it is necessary to be familiar with the circumstances under which palpitation is induced. It may be neurosal, or it may be due to muscular embarrassment. For successful treatment it is essential to be accurate as to which form we have to deal with.

We will take the muscular form first, and get rid of it. It was stated in the second lecture that hypertrophy of the heart was due to increased internal pressure upon the heart-walls, and that more rapid and powerful discharges of the cardiac ganglia were induced by this increased internal pressure. This is achieved by the sensory nerves of the heart conveying the impression to the cardiac ganglia. We there saw that increased motor activity was accompanied by a dilatation of the bloodvessels carrying more nutritive material to the functionally active muscle. Now, the palpitation of muscular inefficiency stands in a most suggestive relation to hypertrophy, and the view of its causation expounded in these lectures. Palpitation, or an excited condition of the heart's action, differing from it in but degree, is induced in most persons by rapidly ascending several flights of stairs. We know that in such effort the blood-pressure in the arteries is greatly raised. We know that an aortic cusp is occasionally torn down by the high blood-pressure of violent effort. Observations made on rowers show that the blood-pressure during one portion of the stroke is

enormously raised, though it falls again at another part. We are familiar with the fact of effort producing aortic valvulitis and hypertrophy. Very well, then, we can comprehend that when the heart is suddenly embarrassed by effort, and the blood-pressure in the arteries rises, the heart is not completely emptied: a portion is left behind on each systole, the blood rushes in from the veins behind, and the internal pressure on the muscular chamber is increased; consequently the condition of temporary distension of the heart is produced. This distension, through the sensory nerves of the heart, leads to more rapid and more powerful discharges of the cardiac ganglia; and thus palpitation or more energetic muscular contraction is set up, and the heart recovers itself. Under such circumstances palpitation is an indication of muscular embarrassment. In a dilated heart palpitation is readily induced by slight causes; it is then an active sign of debility in the heart. This corroborates what was said at the second lecture, that dilatation is a low form of a new equilibrium limiting the powers of the patient. Such palpitation, induced by effort, requires its own treatment. Here rest, limiting the demand upon the heart, is the first point to be attended to. Then it is well to give an agent like digitalis, which excites more energetic ventricular contractions, and so enables the heart-walls to meet the increased internal pressure within them. Such palpitation may often be utilised in practice; and a few turns sharply round the room will often reveal the debility of a heart which beats steadily and rhythmically while the patient is quiet.

Then again, there is the palpitation common in those who are the subjects of gout, or of chronic Bright's disease; and especially women, where, as said before, the hypertrophy is usually blended with dilatation. Here any sudden variation of the calibre of the peripheral arterioles is accompanied by a rise in the blood-pressure in the arteries, the ventricle is embarrassed, and palpitation is the consequence. As said before, the sensory nerve of the heart is also the vaso-inhibitory nerve, and dilates the peripheral arterioles when the blood-pressure within the left ventricle is high. The best method of treating such palpitation is to follow the natural indications, and to dilate the peripheral arterioles with amyl. This drug dilates the terminal vessels just as they dilate in a blush, and so lowers the arterial tension and the blood-pressure within the heart. I made a series of observations on the out-patients of this hospital recently as to the action of amyl. The slow, steady stroke of the hypertrophied heart, and the tense vessels, were at once exchanged for a rapid, light stroke of the heart, with compressible arteries; indeed, a condition of the vascular system like that existing in pyrexia was induced. In the paroxysms of

palpitation in lithiasis, amyl promises to be most useful, relaxing arteriole spasm. The palpitation of hysteria, also due to arteriole spasm, would probably be equally benefited by the inhalation of amyl, a perfectly safe agent in medicinal doses. Of course, the preventive treatment of the palpitation of lithiasis is that given for that condition in the last lecture.

As to the relations of palpitation to a high blood-pressure, the letter I read in my first lecture, from an old patient, a Westmoreland peasant with old-standing mitral disease, contains a most pregnant and suggestive remark. He writes: "I have frequent attacks of palpitation, followed by a feeling of suffocation, continuing from ten minutes to half an hour; they then pass off, leaving nothing disagreeable. I am suffering from a cold I got three weeks ago, and what is most singular, I never have palpitation when in that state." In the pyrexia of a cold the blood-pressure in the arteries is lowered by dilatation of the peripheral vessels; and the square-headed sphygmographic tracing of high blood-pressure in gouty conditions is changed by a cold to the acutely-pointed tracing of pyretic states with low arterial pressure. Now, without supposing that there is nothing more complex in this matter than the variation in the arterial blood-pressure produced by a cold, this fact of no palpitation during a cold is very suggestive as to the cause of palpitation in muscular embarrassment. The peripheral arterioles are dilated, the blood-pressure in the arteries is lowered, and then the crippled heart can contract and expel its contents without difficulty, and so no palpitation occurs. In addition to this there is of course quiescence during the enforced rest of a cold, and thus the heart is not taxed by muscular effort. Taken altogether this statement is exceedingly suggestive as to the relations of palpitation to a high arterial tension, and the effects produced by the lowering of the pressure in the arteries. Indeed, it would suggest that in some states of palpitation where there is valvular disease, the lowering of the arterial tension by the inhalation of amyl nitrite is likely to relieve the heart and do away with the palpitation.

These two forms of palpitation must then, for practical purposes, be distinguished from that form of palpitation which seems a neurosis of the cardiac ganglia. This is the form commonly seen in girls. It is distinguished from the palpitation of muscular incapacity in that effort does not affect it. It is distinguished from the palpitation of lithiasis in that it is not accompanied by any atheromatous changes in the bloodvessels, and the pulse is not firm and incompressible. Broadly speaking, too, it occurs in the sex or at the time of life which are not those of the other two forms. Certainly it is not uncommon at the menopause, especially in women who are

of plethoric habit, and who have dilated hearts. It is often seen well pronounced in girls where there is no history of fright or excitement to account for it, and where one is driven to the conclusion that it depends upon a susceptible condition of the cardiac ganglia—a true neurosis indeed. As such must it be treated. It is necessary to give bromide of potassium or hydrobromic acid, the latter going well with quinine. If the patient be also anæmic, it is well to combine the bromide of potassium with the tincture of the muriate of iron. This combination has, however, a marked tendency to produce constipation in many persons, and so the addition of a little sulphate of magnesia may be indicated. Of course the avoidance of all forms of excitement is desirable. The aggravation of the condition of such patients by a thunderstorm is very pronounced. In almost all cases of palpitation a belladonna plaster over the heart does good.

Closely allied to this form of palpitation is that which must be termed “reflex”—as, for instance, the palpitation associated with a prolapse of the womb, and which is at once relieved by the restoration of the womb to its position by proper surgical measures and appliances. It is found in other forms of uterine disturbance than prolapse. It is also commonly associated with ovarian disturbance, in married women as well as single. Conditions of ovarian congestion, or even of neuralgia, without increase of vascularity, cause a variety of reflex disturbances. We may find it at the bottom of persisting conditions of gastric irritability and dyspepsia, and may consequently formulate the term “an affectionate dyspeptic.” Or it may cause a neurosal cough, or disturbance in the heart, as reflex palpitation. It is the common cause of the intercostal neuralgia so frequent with women. Nor does such reflex disturbance present any difficulty as to the comprehension of its *modus operandi*. Recurrent waves of nerve-perturbation arise or are set up in the ovary; they traverse a series of nerve fibres until they terminate ultimately where the disturbance is felt. Just as when a row of ivory balls are hung in contact with each other in a line, and the end one is struck, it is the last one at the other end which flies from its place. So perturbations arising in the ovary may reach their terminal *locales* in the nerve-endings in the gastric walls, or in the cardiac ganglia, where they cause palpitation; or in the respiratory centre, where they cause a neurosal cough; or in the peripheral endings of the intercostal or facial nerves, where they are felt as gusts of neuralgic pain. This form of palpitation is the most common of the neurosal forms of palpitation, and should always be kept in mind in forming a diagnosis. As to the treatment, it is founded upon the pathology of the affection. The ovarian condition must be attended to by

purgatives, especially salines, and blisters over the affected ovary—for more often one than both are affected. Then those nerve-tracts must be influenced along which the impulses travel, and this is most effectually done by giving bromide of potassium in scruple doses. By such treatment combined relief is soon furnished, except in those cases where the heart is naturally excitable; and in such cases more time must be allowed for the successful treatment, and a less perfect result be looked forward to.

The heart as the seat of the emotions, as the older physiologists placed it, is intimately related with the reproductive organs and the passions. The exercise of the nuptial rite is not uncommonly productive of disturbed action of the heart—not palpitation only, but unrhythmical action accompanied by loss of power. In many cases this does not necessarily involve over-indulgence; but merely that an unusually susceptible heart is perturbed by what in others causes no disturbance. Such is not uncommonly the explanation of a disturbed cardiac action inexplicable in every other way. Many men who live in the country under the most favourable circumstances—clergymen, for instance—suffer from disturbed cardiac action, with a feeling of not being quite well, which has no ready explanation. On accurate analysis it will be found that it is not the discharge of their social, nor yet of their parochial, but rather of their spousal, duties which is telling on them. Of course in these cases the causation must be borne in mind in the treatment, which is usually unsatisfactory. It is a matter too in which the medical man, especially a young one, will do well to tread warily, as the carrying out of his advice too strictly may lead to unfounded suspicions and domestic unhappiness.

Having eliminated these reflex conditions of neurosal palpitation, there remains finally a form of palpitation which is as truly neurosal as whooping-cough. Whooping-cough is a neurosal disturbance of the respiratory centre—an explosive discharge of the centres which preside over the respiration. In the same way we may have neurosal disturbance in the cardiac centres manifesting themselves in bouts of excessive nerve-discharges leading to violent paroxysms of palpitation. Here the action of the heart is very violent for the time, but the pulse is not affected, and the tumultuous excitement of the central organ is not communicated to the rest of the vascular system. In such cases as these the line of treatment to be followed must, in the present state of our knowledge, be truly tentative. It may be found in some cases that tonics—steel, quinine, or strychnia—are indicated; while in others, sedatives, as bromide of potassium, are rather suggested. In prescribing for whooping-cough and asthma we now study the condition of the respira-

tion betwixt the paroxysms; so, in genuine neurosal palpitation, we shall probably find that careful observation of the condition of the vascular system ordinarily will suggest the appropriate treatment for the neurosal paroxysms.

Where the heart's action betwixt the paroxysms is excited, then probably bromide of potassium is the drug most likely to be of service. On the other hand, if during the interval the pulse is compressible, and the heart's action quiet and steady, then belladonna or strychnia, with or without a little digitalis or iron, seems rather to be indicated. In the production of these intermittent attacks of palpitation we do not yet know what part is played by the vaso-motor nerves. It seems probable that in some cases of neurosal palpitation, the starting-point may possibly not be so much in the cardiac ganglia as in the alteration of the calibre of the small arteries from vaso-motor disturbance. A sudden alteration in the blood-pressure within the arteries from arteriole spasm would, we can conceive, be very likely to alter the heart's action from the obstruction offered to the complete contraction of the ventricle in systole. We all know that there are two neurosal conditions in which excited and not rarely violent action of the heart is found. These are chorea and Graves's disease. In both of these affections the heart will beat in such a manner as to cause a tremendous impulse in the thoracic walls; indeed, the Germans use a word corresponding to our word "shattering," because the violent action of the heart suggests the idea that it will shatter off the chest-walls over it. As to how the heart becomes thus affected in these two maladies we do not yet know. Whether there is a loss of control of the higher centres, some paresis of the vagus and loss of that inhibitory or controlling action of this nerve over the discharges of the cardiac ganglia, or not, cannot yet be affirmed; or whether there is a neurotic condition of the cardiac ganglia, so that they for the time escape from the control of the vagus, may not yet be dogmatically stated. Trousseau describes Graves's disease as "a neurosis of the sympathetic," suggesting idea that the malady starts in some disturbance of the sympathetic nerve and its ganglia, rather than in any modification of the control exercised by the vagus. Certain it is that in palpitation associated with chorea, bromide of potassium combined with iron gives the best results, perhaps, of any plan of treatment. In Graves's disease no treatment is very satisfactory, as it is a most intractable condition; and, probably, of all forms of palpitation, that associated with Graves's disease is the one for which we can do least, and where any temporary good achieved is most fleeting and least durable. The treatment of the palpitation in these conditions is the treatment of the general state, and for that each case must be studied separately and therapeutically approached according to its individual indications.

In considering the neurosal affections of the heart we must face the broad question, "Are the nervous disorders of the heart on the increase at the present time?" And there is no doubt an affirmative answer must be given to this question. The forms of palpitation described above must have existed from all time; and the bosoms of the spouses of primitive man must have vibrated from the violent action of the heart;—ages before advancing civilisation had rendered the nervous system of women eminently susceptible, and given to it a delicacy of organisation which has long been the characteristic of refined women. It may be questioned how far there is any increase in the neurosal affections to which women are liable beyond what is fairly attributable to the extending habit of tea-drinking to excess. But as to the increasing liability of men to have nervous disorders of the heart there can be no doubt; this is due chiefly to the exciting circumstances under which many men now live—circumstances which increase the taxation of the nervous system. We know that diseases of the nervous system, including insanity, are at the present time greatly on the increase; and good authorities like Dr. Crichton Browne tell us prophetically that such diseases must increase in the future as the battle of life falls more on the nervous system, and less on muscles, bones, thews, and sinews. The observations of Dr. Da Costa of Philadelphia during the late American Civil War bear out what has just been said. He observed a form of heart disturbance then seen by him, which has not been described by any previous writer. This form of neurosal affection of the heart he denominates "Irritable Heart." He found it usually among men who had been engaged in active service, and who got either an attack of ague or of diarrhoea, something indeed which pulled them down. After a short stay in hospital the patient would rejoin his command, but soon found that he was not equal to his work as before. He would be attacked by dizziness and palpitation when on the march, so that he had to be invalided. The heart's action would be excited and irregular, resembling that condition previously spoken of which I said might be called "a badly behaved heart." No doubt the condition was set up by the excitement of that remarkable war acting upon a people whose nervous system is highly strung—that is, speaking of them as a nation. Indeed, the whole circumstances are merely a foreshadowing of the general condition of life and nervous system which we seem steadily approaching. In this condition there is a mobile pulse readily affected by posture, varying from twenty to forty or more per minute when the recumbent posture is abandoned and the patient stands up; the respiration not being correspondingly affected. Then exertion is badly borne by them. There are

also concomitant nervous affections, with a tendency to free perspiration, especially of the hands. It did not seem associated with excess in tobacco, or sexual excess, but rather with excitement and fatigue. It is a very intractable condition, and Da Costa says the treatment is never a short one. As to the line of treatment to be adopted, he states that when there was concomitant hypertrophy, then aconite was most useful, as lowering the excited action. But when there was debility of the circulation, then digitalis, belladonna, veratrum viride, or strychnia, were most serviceable. In fact, Dr. Da Costa found out thoroughly what has just been said about the neurosal affections of the heart, that the general conditions accompanying the cardiac disturbance must be our guide to the treatment of each individual case. This "irritable heart" is comparatively rare with men, but several distinct cases have enabled me to verify Dr. Da Costa's observations and description.

The next matter to engage our attention is that disturbance of the heart's action known as *irregularity*. Here there may be no palpitation, or only on exertion, but in the midst of rhythmical contractions the heart makes a pause, a brief halt, which does not extend to a true intermittence of a beat. Dr. B. W. Richardson describes it thus:—"It is like a smith who, striking on a forge a number of strokes in rhythmical succession until tired, changes the action for a moment to give a more deliberate and determinate blow, and then rings on again in regular time." To me it has resembled the change of a horse's feet when cantering, when commencing, for a change to lead with the other foot first. It is commonly found along with dilatation, and gives one very distinctly the impression of the heart rolling over and then going on again in rhythmic measure. Sometimes this halting beat is of unwonted force, as if the longer it had been kept back the more powerful it grew. George Balfour in reference to this phenomenon quotes the lines of Ballatyne—

"My vera heart goes loup, loup,
Fifty times a day."

And this describes very accurately the condition of the cardiac action in an anxious mother with a feeble heart. Such irregular action is a true nervous phenomenon, but, nevertheless, one rarely found without some impairment of the muscular walls of the heart. It is therefore of worse prognostic import than simple palpitation. Whenever it is found, and along with it there is palpitation on slight effort, it is pretty certain that there is some dilatation about the cardiac cavities, usually with thinning of the walls. Sometimes it is found every fourth or fifth beat, and in other cases at every fifteenth or twentieth. Broadly it may be said, the more frequent it is, and the more

readily it is increased on slight exertion, the more significant it is. The longer the rhythmic intervals, the less serious its indications, so that it may be little more than a nervous phenomenon allied to intermittency.

Intermittency is a phenomenon of great importance practically, and a thorough and complete understanding of it is very desirable. To B. W. Richardson are we indebted for the full exposition of its being in many cases a purely nervous phenomenon. He would hold that it is exclusively a nervous phenomenon, and that it has no relation whatever to any organic change in the heart. In this I cannot quite agree with him. The intermittent stroke must of course be due to some modification of the rhythmic discharges of the cardiac ganglia; if the motor explosion was discharged muscular contraction would follow. Consequently we are compelled to admit that it is a nervous phenomenon; but while doing so I think its diagnostic and prognostic value depends upon its concomitants. Thus in persons comparatively young and otherwise perfectly healthy, the intermission of a beat in their pulse, even at rhythmic intervals, is a matter of little moment. It is a mere nervous disturbance, and has no practical value. The halt does not produce any alteration in the blood-pressure in the arteries, and therefore has no evil effect upon the economy. It may be dismissed as in itself being of no moment. Dr. Richardson has given a series of cases where this intermittency has been produced emotionally. Thus, in one case it was induced by terror during shipwreck, and occurred four or five times per minute; at first the patient was painfully conscious of it, but in time it fell to two in the minute, and he ceased to be conscious of it. In another case it was induced by grief, and when the case was first seen the intermissions were no fewer than twenty-three in the minute. Under treatment she recovered perfectly for all practical purposes, the halt only occurring in one out of 160 beats. In another case it was induced by passion. In a fourth it was induced by fatigue, mental and bodily. Here it returned whenever the patient was overworked or emotionally excited. In one case it was congenital, and was found in an infant on the day of his birth, and continued until it was five years old, when it gradually disappeared. From two cases referred to by him it would appear that cardiac intermittency may, to a certain extent, be hereditary, or a mere family peculiarity. But even Dr. Richardson says—"In old age intermittent action of the heart is exceedingly common." It is indeed associated with senile changes; and though I know a number of medical men of advanced life, who have had an intermittent pulse for years, and who still are hale; nevertheless, in many other cases I have found it asso-

ciated with organic changes which in no long time ended in death. Dr. Richardson says—"The intermittency of the arterial pulse occurs from an independent failure of action of the left ventricle of the heart. The ventricle continues in diastole for two or more strokes of its systole." This is probably the explanation of the phenomenon; and from some observations made on rats at the time I was the Senior Resident Medical Officer of the Leeds Dispensary, I can say that when the chest was opened and the animal was dying, there was distinct intermission of the ventricular beat; at first, the ventricular contraction was set up by the second auricular contraction, no ventricular systole following one auricular contraction; but as the animals died, a third and even a fourth auricular contraction occurred before the movement in the auricle was continued into the ventricle. Probably the same thing occurs in man, not only in the intermittent pulse of the moribund condition, but in that which is found as a permanent condition, especially in the aged. While maintaining that it is a nervous phenomenon, Dr. Richardson writes—"The man or woman with a hesitating heart is thereby unfitted for sudden tasks, demands, resolves, which, when the heart is firm, are considered as of comparatively little moment; for when the heart hesitates, the brain, which reposes for its power on the blood the heart supplies to it, falters with the heart, just as the gas flickers when the steady pressure is taken off the main." In long protracted halts, lasting over four ordinary beats of the heart, of which I have seen two cases, the sensations of the patients were terrible, as their features testified as well as their tongues. When the halt is so long that the brain is insufficiently supplied with blood, the intermittency is a malady; when an occasional intermittence occurs, but does not affect the blood-pressure in the arteries, then it is of no gravity. In one case of distinct intermittent pulse in a gentleman under my care, it was diagnosed as depending on an insufficiency of sleep; and proper hours of sleep soon put an end to the phenomenon.

Dr. George Balfour says of simple intermittence—"It is sometimes only an early indication of failure of cardiac power dependent upon anæmia, overwork, or worry, or upon valvular disease or gout, but it is often a purely nervous phenomenon." He also notices the thump or "loup" which follows the intermission; it is a powerful contraction of the ventricle after its momentary halt. In some cases of sudden death due to emotion, he believes the cause of death is a permanent halt of the ventricle. He goes on to say—"At other times, partly from the intensity of the impression, but chiefly from debility of the nervous system, this 'loup,' thump, or intermission, of which the loup is the most striking subjective symp-

tom, not only occurs under the instantaneous excitement of any emotion, but repeats itself, at first at shorter, afterwards at longer intervals, until at length it dies out under the re-assertion of the normal condition of the nervous system. Now and then, however, it never dies out, but repeats itself so long as life continues." He relates a case, that of an old gouty lady, who was attacked with syncope, where he found the radial pulse only twenty to the minute. On examining the heart, he found it beating "with perfect regularity, but with unequal force, so that the apparent abnormal slowness of the pulse was simply due to the fact that only about every third beat reached the periphery." Such is certainly the fact in some cases of intermittence in old persons; there is not a complete halt of the ventricle, but a systole so incomplete and feeble that it does not reach the radial pulse.

Finally, all knowledge commences by vague impressions, often erroneous, and not rarely misleading and unfounded. But these imperfect impressions are the pioneers of truer knowledge; just as the view of Sir Dominic Corrigan—that the utility of digitalis in aortic stenosis lay in its slowing the ventricular contraction so that more time was allowed for the blood to pass the narrowed ostium—though now known to be erroneous, led to more careful observation, and a truer knowledge of the action of digitalis. We now know that the good effected is by setting a more powerful contraction against a narrowed orifice, so that a normal amount of blood is thrown into the aorta each minute, and so the wants of the system are met, and the stenosis compensated. So the views set forth here may need modification and correction, and yet in so doing add to the sum total of our positive knowledge about the diseases of the heart. As the mystery surrounding them melts away, and the clouds of imperfect information are rolled forth to make way for the more perfect light of day, so we shall find that, instead of occult conditions which we can detect, but not explain, we shall be able in each case of heart disease to understand its causation, and the direction in which it is moving, and so do much both in the way of prevention and of retardation; and may often usefully aid the natural efforts at compensation by our knowledge of and acquaintance with them and how they are brought about.—*Med. Times and Gazette*, Dec. 7 and 14, 1878, pp. 647, 675.

32.—ON THE USE OF ARSENIC AS A BLOOD AND CARDIAC TONIC.

By Dr. STEWART LOCKIE, Senior Physician to the Cumberland Infirmary.

Certain it is that, in cases of anæmia approaching in gravity the so-called essential or pernicious anæmia, arsenic is capable of

producing great benefit. In support of this statement, I venture to submit the following cases.

Mr. J. W., aged 67, consulted me during last year on account of increasing weakness and languor, occasional vertigo, palpitation on ascending a hill, flatulence, and rushing noise in the left ear. There was marked pallor of the skin. A systolic *bruit* was heard at the cardiac apex. There was no enlargement of the liver or spleen. On microscopic examination of the blood, there was found no increase of white corpuscles, while many of the red were irregular in shape, many of them being ovoid. On subsequent inquiry, it was found that he had internal bleeding hemorrhoids, and these were ligatured in December by my friend Dr. Maclaren. After this, he improved a little in appearance and strength. The hemorrhage was stayed. Iron in the dialysed form was administered. But the improvement did not continue.

On February 18th, 1878, there had been no bleeding since the operation; but the patient was now paler than ever, and was unable to move about outside. He complained of weakness, languor, faintness, and breathlessness. The cardiac dullness was increased transversely. There was a systolic *bruit*, whose maximum intensity was in the mitral area, and was propagated to the left; and a second, the maximum intensity of which was in the tricuspid area; there was also a well marked systolic *bruit* in the pulmonary area. There was no impairment of resonance over the pulmonary organs; but the respiratory murmur was harsh in the left subclavian region. There was oedema of the lower limbs, and a complaint of numbness and tingling in the tips of the fingers and soles of the feet. This abnormality of sensation was not felt until after the operation. The urine was acid, of specific gravity 1019, and contained no albumen.

February 22nd. The ophthalmoscope showed in the left eye, near the apparent inner and lower margin of the disc, a small extravasation close to a vessel. The fundus of each eye was pale. Under the microscope the blood was found to be watery, the red corpuscles relatively much diminished in number; there was no increase of colourless corpuscles. The red corpuscles formed themselves into irregular masses, only a few rouleaux being seen in the field. Many of the corpuscles were somewhat oval in form; some were pear-shaped, while numbers presented one or several mammillated processes; as a rule, only one such process. Most of the red corpuscles appeared somewhat larger than normal, and presented a swollen appearance, while others were less by one-half than normal. At this date, he was ordered three minims of liquor arsenicalis three times a day, with no other medicine.

On February 26th, the dose of arsenic was increased to five minims. He thought himself stronger. The anæmia did not seem to have increased. The appetite was good. There were no new symptoms.

After this date, a slow but gradual improvement took place, and, on April 23rd (two months after commencing the arsenic), he had improved very much. The colour of his face was very good. The conjunctivæ were still a little pale. He felt much stronger, and had no dyspnœa on ascending a hill. There was still some œdema of the lower limbs. The coloured corpuscles were much more uniform in size and normal in shape, and collected much more into rouleaux.

May 20th. The cardiac murmurs had disappeared, and there was now no œdema of the limbs. The patient looked well, and had a good deal of colour. He was to cease taking the medicine.

Subsequently, I met this patient in the street, and he informed me that there had been a slight return of hemorrhage; this, however, speedily subsided under a few doses of glycerine. Last month, the wife of the patient informed me that her husband had been lately in Edinburgh, and had climbed to the top of Arthur's seat, thus testifying to the completeness of the recovery.

It is to be noted that, after the operation on the piles in December, this patient improved somewhat; but that, in February, without any return of bleeding, he became more anæmic than ever. Had it not been for this, one would have concluded that the bloodlessness was entirely due to the hemorrhage. This conclusion, however, in the circumstances, is untenable. There was evidently some other cause at work, and, without positively averring that it was a case of essential or pernicious anæmia, it was certainly a grave form of the disease which iron failed to benefit, but over which arsenic had remarkable power. Coupled with Dr. Bramwell's cases, the result was such as to encourage one to try the remedy in any future case.

My next case is that of a married lady, somewhat beyond the middle period of life, now residing in Edinburgh, who consulted me as she passed through Carlisle on June 18th, 1878. For two or three years, more or less, she had been troubled with breathlessness, occasional palpitation, and great languor, so that she was incapable of sustaining any exertion, either mental or bodily. She stated that she sometimes had nausea more or less constantly for weeks together, and frequently vomiting of food, sometimes vomiting after every meal, and sometimes once or twice a day. She had had a fibroid tumour of the uterus for many years. This at one time was the seat of

pain, but latterly had ceased to trouble her. Menstruation had ceased eighteen months before the date of my visit. Very occasionally—only once or twice in two months—she had slight bleeding from hemorrhoids. She stated that she had not progressively become worse, but sometimes became a little stronger, when the vomiting would cease. She had just returned from Buxton, where she had been with her husband, who was also an invalid. Whilst there (five weeks), she had had vomiting nearly every day. The vomiting especially occurred if she made the slightest exertion after taking food. There was no gastric uneasiness. She had consulted a medical man in Buxton, who had advised her to go to a German watering-place. This, however, she was very unwilling to do, if it could possibly be avoided, and indeed she did not seem to me fit to undertake the journey.

On examination, I noted the following:—"The patient is very pale and anæmic-looking, somewhat sallow; the conjunctivæ are pale and a little yellow. There are systolic *bruits* in the mitral, pulmonary, and aortic areas, with accentuated pulmonary second sound; venous pulsation in the neck; no *bruit de diable*. The cardiac dulness is somewhat increased transversely."

On microscopic examination of the blood, the white cells were not found increased; the red corpuscles varied much in size and form, many being ovoid and some caudate. There were no abnormal percussion or auscultatory phenomena in the lungs. Hepatic and splenic dulness were normal. There was a firm nodulated tumour in the hypogastric region, twice the size of an orange, movable, found on vaginal examination to be connected with the uterus. The urine was of amber colour, acid, of specific gravity 1010, and contained no albumen, bile, or sugar.

The patient had taken iron in various forms, but with no permanent benefit. Two years and a half ago, when she resided in Carlisle, she took Kirby's pills of phosphorus, iron, and nux vomica, from which she thought she derived some degree of good.

I prescribed for her liquor arsenicalis, beginning with three drops three times a day, the dose to be increased or diminished according to circumstances; and ordered her to take glycerine in two drachm doses once or twice a day, if at all constipated; to rest in the recumbent position after each meal; the meals to be small and frequent; to take stimulants only so far as they helped her to take food.

In a fortnight, I received a note from this lady, in which she said: "I am pleased to tell you that I have improved steadily, though slowly. During the last three days, I have been able

to take a fair amount of food, and I drive out, besides sitting in the garden for at least an hour daily. I now take six drops of arsenic at each dose, and it appears to suit me. *I am said to have almost a pink tinge in my skin.* Will you let me know if you think any English waters would be of real advantage to me, and which? I do not intend to go abroad this year, and shall prefer merely to go to the country." This was dated July 1st. On the 17th, she again wrote, saying she was going to Harrogate. She said: "I continue to improve slowly but steadily. My walking powers are the worst. My appetite is good, and I do not feel any need for wine. I have no feeling of nausea, and can eat vegetables and fruit. . . . *The colour of my face and hands is becoming quite natural.*"

On August 27th, I saw this lady, as she returned from Harrogate to Edinburgh, when I took this note. "She continued the medicine until five weeks ago. She had improved much in strength and appearance, and the sickness had ceased. Whilst at Harrogate, she had an attack of erythema of the leg, and she then discontinued the arsenic, as the medical man whom she consulted thought it might be caused by it. The leg became well, but is now bad again, without her having resumed the medicine. She drank the chalybeate water at Harrogate. She now feels immensely better and able for exertion. Her complexion is almost ruddy; the appetite good; the tongue and inside of the lips raw (she is subject to this). There are no cardiac *bruits*, excepting a very faint one in the pulmonary area.

In 1871, the late Dr. Anstie, whose premature death the profession so deeply deplores, published a remarkable paper on the Pathological and Therapeutical Relations of Asthma, Angina Pectoris, and Gastralgia. In this paper, he says:—"There is one remedy which is supremely effective, where it can be tolerated, in all these three maladies; namely, arsenic." Now, it is this element of pain in cardiac disease which has been usually, though not exclusively, looked upon as the indication for administering this drug, and there is a concurrence of experience as to its value in this respect. But I am convinced that other indications may be fulfilled by the use of arsenic. I believe it to be a valuable tonic to the cardiac muscle, and that it forms a useful adjunct to digitalis in ordinary valvular disease of the heart, where there is failure of compensation, with its consequent results. Further, it seems to be of great value even in fatty degeneration. One case of, I believe, undoubted fatty heart seemed to derive much benefit from the use of this remedy. It was that of an innkeeper, aged between 40 and 50, who, though at the time of which I speak of temperate habits, had at one time been addicted to excess. I was called

to him on February 9th of this year, on account of an attack of renal colic, which was relieved by hypodermic morphia. He had previously been suffering from attacks of giddiness. These continued after the pain was relieved, and especially came on when assuming the upright position or when he sat up in bed. The impulse and first sound of the heart were very feeble, and the face was pale and somewhat sallow.

Digitalis in small doses was ordered, and ammonia and ether as stimulants. The pulse became very slow (below 40) and irregular, and continued so after the withdrawal of the digitalis. Liquor arsenicalis was then ordered instead, and gradually the pulse rose in frequency and became regular. His colour became much better, and the attacks of giddiness quite passed off. He continued to take the arsenic for some time, and, on May 28th, this note was taken:—"Cardiac impulse not strong but fair. First cardiac sound somewhat feeble at the apex; almost inaudible in the aortic area. Pulse 76 to 80. Looks and feels well."

I have not yet had an opportunity of testing the power of the remedy in other cases of fatty heart, but shall not fail to do so when the opportunity occurs. I am aware that late experiments tend to show that fatty degeneration of the muscular tissue of the heart is one of the results of feeding animals with arsenic; but this is no reason why the drug, administered in small doses, should not be beneficial in cases of the disease in question. We cannot safely infer from the effects of large doses of drugs what will be the result of the administration of small ones. Of some remedies, we know that the effects of large and small doses are not only dissimilar, but opposed; witness the vomiting induced by large doses of ipecacuanha, and the speedy relief of some forms of vomiting by small ones. The very drug we are considering, as we know, causes vomiting in large toxic doses, while in minute doses it is the remedy *par excellence* for at least one form of vomiting—the vomiting of drunkenness.—*British Medical Journal*, Dec. 7, 1878, p. 828.

33.—ON BELLADONNA AS A CARDIAC AND RESPIRATORY STIMULANT.

By the EDITOR of the MEDICAL PRESS AND CIRCULAR.

Although the discovery of our most valuable remedies and their uses has been hitherto chiefly due to accident and empirical observation, the time seems to have arrived when our knowledge of the action and uses of drugs in the treatment of disease will be much extended by exact experimental research. This is evident from the great light that has been, within the last few years, thrown upon the physiological action and uses of certain drugs about which we formerly knew very little, and the

employment of which was restricted to one or two diseases. *Digitalis*, for instance, has been long known as a remedy in dropsies, but our knowledge of its *modus operandi*, of its action upon the heart, and of the kinds of dropsy in which it is most useful, is comparatively of recent date. In the same manner a more exact method of therapeutical inquiry has brought the action and uses of belladonna into more prominent notice than it has hitherto received from the profession. While years ago belladonna was chiefly used as an external agent, and internally was but partially recommended, and still more partially used, in one or two diseases only, such as whooping-cough and asthma, it is now found to be a very serviceable drug in many other complaints.

It is not, however, our intention to notice the marked influence of belladonna over the secretion of milk, and over the perspiration of phthisis, nor to dwell upon the antagonism existing between it and opium, but rather to notice its action as a cardiac and pulmonary stimulant, and the cases in which, in virtue of this action, it may be successfully employed.

Belladonna has a marked action on the heart and circulation. In moderate doses it raises the blood-pressure in the arteries, partly contracting the peripheral arterioles, and partly by its direct action upon the heart. Dr. John Harley believes that to this action the delirium of belladonna poisoning is to be attributed, and that, in consequence of this same action belladonna is the best direct diuretic we possess for increasing the bulk of the urine. Perhaps, however, the action of belladonna upon the heart and circulation is best illustrated by the experiments which Prof. Fraser made on the effects of that drug in arresting death from Calabar bean in lethal doses. When the physostigma was administered, the number of cardiac beats and of the respirations fell very markedly, but upon the injection of belladonna they rose first above the normal rate, and then again fell to it. After the administration of the Calabar bean to a rabbit the heart's contractions gradually fell from 40 to 32, 22, and ultimately 11 per ten seconds. Some sulphate of atropine was then injected, and in one minute the heart's contractions had risen to 56 per ten seconds, in seven minutes to 60, after which it fell to its usual rate. It may be said, therefore, that belladonna increases the blood-pressure, probably by stimulating the vasomotor centres, and that, in consequence of this action, we can partly account for some of those objective symptoms usually observed when large doses of the drug are taken. For at first the pulse is slackened, hard, and resisting; then it becomes full, strong, and frequent; the temperature is slightly raised; the countenance is red or flushed; the ocular conjunctiva is brilliant, and the pupils dilated. We say, "partly account for" advisedly,

as, no doubt, some of these symptoms are not so much due to an increase in the blood-pressure, as to a reactionary dilatation of the walls of the arterioles, and the admission into them of a larger supply of blood.

Nor is it the circulation only which is affected by belladonna. The observations of Prof. Fraser, Dr. Harley, Dr. H. C. Wood, and Dr. Fothergill, show that this drug has a decidedly stimulating effect upon the respiratory centres. In the experiments of Prof. Fraser, the respirations fell from 20 to 16 in three minutes after the injection of the physostigma; in 28 minutes "there were only infrequent laboured respirations occurring at irregular intervals." After the injection of the atropia, the respirations rose to 15 in two minutes, in six minutes to 20 again; and in an hour and a half had settled to 15 per 10 seconds. Similar facts were observed by Prof. Bennett in his report to the British Medical Association on the antagonism of belladonna and Calabar bean; while Dr. Fothergill's observations on the antagonism of belladonna and aconite, show that in aconite poisoning the respirations fail, and that while digitalis does not affect the respiration sufficiently to prevent that failure, belladonna does. After the administration of belladonna, the respirations which had been failing under the aconite recovered again, until they reached a normal point. Hence, when death is approaching from paralysis of the respiration, induced by a poisonous agent, belladonna restores the respiratory efforts, and so saves the animal from death (Fothergill). We may also remark that besides atropia, strychnia and ammonia also act powerfully upon the respiratory centres when these are being paralysed by disease.

We now come to the practical import of these facts, and to the explanation which they afford of other facts not hitherto intelligible. In the first place there is every reason to believe that the antagonistic effect of this drug in cases of poisoning by opium is due to its influence on the circulatory and respiratory centres. As Dr. Reinhard H. Weber, and others, have remarked, the danger to life in these cases comes principally from the steadily increasing paralysis of the respiratory centres. Hence the great stress which our best authors lay on artificial respiration after all other means have failed. If we can keep up the circulation and respiration, there is a good chance of recovery, especially as the kidneys (excited to action by the belladonna) are in the meantime constantly removing some of the poison from the blood. From these facts, therefore, taken in connection with the experiments above mentioned, we may conclude that the antagonistic action of belladonna to that of opium depends upon its exerting a contrary influence on the circulatory and respiratory centres to that produced by the latter drug, namely,

a restorative and stimulating one. It may also be inferred that belladonna will prove to be equally useful in other poisons which kill in the same manner as opium does.

Moreover, the question arises whether this restorative effect of belladonna upon a paralysed state of the respiratory centres may not be utilised in those affections in which the respiratory functions are disturbed or seriously imperilled; and a few cases have been recently reported which would appear to justify such a supposition. Dr. Reinhard H. Weber, who has given much attention to this subject, relates in the *Philadelphia Medical Times* for August a case of heart disease in which the severe dyspnœa and other symptoms were, in a great measure, removed by belladonna when other remedies had failed. The patient suffered from "stenosis and insufficiency of the aortic valves; there was great œdema of the lower extremities, and the orthopnœa was so great in the night that he was obliged to sit in his bed propped up with pillows. He had been under the care of a regular physician three months before Dr. Weber saw him, but neither his dropsy nor his other symptoms had been much relieved. To be brief, the belladonna treatment was very successful, and in about ten days from its commencement the patient was able to breathe and sleep comfortably. His kidneys acted well, the dropsy disappeared, and he was able to resume his work.

A similar case is narrated by Dr. Fothergill in his recent interesting work on "*The Antagonism of Therapeutic Agents.*" The patient had a complication of lithiasis, heart disease, chronic bronchitis, and irritable bladder. His nocturnal attacks of dyspnœa had become very distressing, and his nose, cheeks, and hands were of a blue colour "from the paralysis of his peripheral arterioles by the chloral" which he had been in the habit of taking. Under ammonia, strychnia, and digitalis, he greatly improved, but the strychnia so excited his irritable bladder that it had to be stopped, "and belladonna, which exercises the same influence over the respiratory centres, but which lessens the activity of the bladder centres, was given instead." Under this change of treatment the improvement was steady and gratifying. All his symptoms were removed, even the mitral murmur vanished, and his heart began and continued to beat steadily. It might be said that in the first case, the recovery was due to the diuretic properties of the belladonna, but Dr. Weber's patient had taken diuretics alone without any improvement, while, in Dr. Fothergill's case, there was no dropsy present.

Dr. Weber even goes so far as to recommend this medicine as part of the treatment in all cases of collapse or shock in which danger threatens, from failure of the circulation, no matter

what the disease is; and he has actually resorted to it with success in severe cases of collapse occurring during attacks of cholera, scarlet fever, typhoid fever, &c. Nor should it be forgotten that belladonna has been long known as a valuable remedy in asthma and hooping-cough, its efficacy in these cases being no doubt partly due to its peculiar physiological action. Belladonna was a favourite remedy in the treatment of asthma with the late Dr. Hyde Salter, and Meigs and Pepper, with numerous other writers before them, have spoken highly of it in the treatment of hooping cough. In fact, this drug would, in our opinion, have been found more generally useful had not practitioners been afraid of prescribing it in sufficient doses to secure its peculiar action upon the system; and perhaps this fear may yet stand in the way of belladonna and other potent remedies having a fair trial in those diseases and conditions of the system in which recent inquiries have shown them to be useful. The reader, therefore, might take a hint from some remarks which a French physician lately made on the subject. In a clinical lecture "On Belladonna," delivered at the Hospital for Sick Children in Paris, M. Jules Simon said: "You will, gentlemen, easily see by the countenance of your patients the effects of the drug; and you ought when prescribing this medicine to bear in mind the principle by which I am generally guided, namely, that of repeating and increasing the doses of therapeutic agents until the physiological effects of the drug are well marked. Just as in treating syphilis of the adult you ought to administer mercury until the first appearance of tenderness of the gums and of salivation shows that the buccal mucous membrane is affected, in the same manner you ought to push the administration of belladonna in children until you see their face flushed and animated, their eyes brilliant, and their pupils dilated. Then you should suspend the administration of the remedy by gradually diminishing the dose; for whilst digitalis may accumulate in the system, belladonna is rapidly eliminated by the kidneys, and the symptoms of intoxication which it causes is essentially evanescent."

We might also allude to the probability of belladonna being useful in many other acute and chronic affections where the respiration is failing—to its use in melancholia and dementia (Crichton Browne)—and to its utility in combination with other medicines, in accordance with a principle now frequently acted upon, namely, that of getting rid of a dangerous or objectionable action of one drug by combining it with another. The scope, however, of this article will not admit of any further remarks on the subject; and we trust enough has been said to show the importance of the reader paying some attention to the researches that are now being made with respect to the action

and uses of this and other valuable agents.—*Medical Press and Circular*, August 28, 1878, p. 169.

34.—ON THE USE OF PYROGALLIC ACID IN INTERNAL HEMORRHAGES.

By Dr. AGMONDISHAM VESEY, Rostrevor.

Rostrevor being a well-known resort for those who seek sheltered winter quarters, I have had unusual opportunities of treating cases of hæmoptysis, &c. Of late one preparation has proved of such signal advantage that I wish to record briefly a few cases illustrative of the treatment.

In certain photographic processes pyrogallic acid is an active agent, and one cannot avoid getting more or less of the acid mixture on the hands, and from the remarkable astringent effect produced I was led to infer that it ought to be of service in the treatment of internal hemorrhages.

The first case in which I used the pyrogallic acid was one of chronic phthisis. I was hastily summoned to see this lady, Mrs. W. I found that she had slight hæmoptysis, and was very much alarmed thereby. The acid was ordered in grain doses every half hour, and in two hours the hemorrhage was arrested. In January, 1877, a more severe attack yielded promptly to the same treatment.

The second case was also a phthisical one. In this the hæmoptysis was very copious. Grain doses of pyrogallic acid were prescribed with the happiest result after a few doses had been taken. By way of precaution in both cases, a dose was given every four hours as long as the expectoration was tinged. This second case was in March, 1878. The patient has again returned to pass the winter here. No return of the hæmoptysis occurred, and there is considerable improvement in health.

The third case was that of a lady who, inheriting the hemorrhagic diathesis (her daughter also being a bleeder), always required a tampon at the catamenial period, so copious was the flow. She underwent special treatment with no permanent benefit. Some five years ago she went to reside in England, and but recently returned to this neighbourhood, when she again came under my care for the old malady. I tried with her grain doses of pyrogallic acid with *M. 40 extracti liquidi ergotæ* every second, third, or fourth hour as necessary during the flow, with the effect of notably diminishing the flux and bringing it within normal limits. All other internal treatment had previously failed to give any relief. She has now tidied over five or six periods, *tuto, cito, et jucundè*.

The last case in which I had an opportunity of using the acid was that of a pensioner who had served in the Crimea, and

had suffered from malaria in India. It was ascertained that at one time he drank spirits to excess. I saw him one morning in September last, and found that he was collapsed and pulseless at the wrist. He had vomited a very large quantity of clotted blood; he had tarry motions passed under him involuntarily. Immediately ether was injected hypodermically, and a mixture of iron and ergot prescribed; the stomach rejected this. Pyrogallie acid was then given in grain doses at short intervals, and subsequently a small dose of morphia. In three hours the hæmatemesis was stayed and did not return. Melæna continued for several days; therefore he was ordered a mixture containing dialysed iron, liq. bismuthi, pyrogallie acid, morphia, in chloroform water. Under this he made daily progress, and left Rostrevor very much stronger than he had been for two years. Previously he had four or five similar attacks. His liver was abnormally small.

Pyrogallie acid appears to me to have the following advantages:—The dose is small; it does not disarrange the stomach in the way that the usual gallic or tannic acid mixtures do; it does not cause vomiting, as iron and ergot mixtures sometimes do; it is easily taken, and has no disagreeable after-taste. It appears to be more rapid and certain than any of the remedies mentioned above, and far surpasses the time-honoured acid infusion of roses, or pil. plumbi cum opio. It dissolves readily in water or in spirit. A spirit solution of definite strength affords a convenient and ready method of administration. There is no reason why it should not be also used in the form of spray in hæmoptysis, but I have had no experience of its use in this way. The dose may also be increased according to circumstances.

In the Irish Hospital Gazette I recorded some cases of hæmoptysis, &c., in which liq. extract of ergot was used hypodermically with success. A combination of the ergot and pyrogallie acid will afford a very powerful means of arresting internal hemorrhages.

Several standard works of reference have been examined without finding any record of the medicinal use of pyrogallie acid; but—"there is nothing new under the sun."—*Dublin Journal of Medical Science*, Dec. 1878, p. 470.

DISEASES OF THE ORGANS OF RESPIRATION.

35.—ON CHAULMOOGRA OIL IN PHTHISIS.

By Dr. I. BURNEY YEO.

The oil when cold is solid, and it is necessary therefore to mix it, after warming, with some other diluent oil. I have used

almond oil for this purpose, as it seemed to me desirable if possible to avoid complicating the effects of the chaulmoogra oil with those of cod-liver oil, with which it has been recommended that it should be mixed. The oil has a nauseous odour and taste, and when mixed with cod-liver oil forms a mixture to which I find most patients are very averse. I should also premise that the cases in which I have given it were those of hospital out-patients, and that is the reason why some of them passed rather abruptly from under observation; an untoward circumstance which one must always be prepared for in dealing with persons of that class.

Case 1.—This case was a very suitable one for trying the effect of a new remedy, as the disease was in an early stage and limited to a small portion of lung. F. J., a pale youth of 17 years of age, whose father and mother had died of phthisis, came to Brompton Hospital as an out-patient on the 2nd Oct. last, complaining that he had twice within the preceding ten days coughed up a small quantity of blood, about four ounces each time; besides the cough and slight emaciation he had nothing further to complain of. There was dulness over the right apex from the clavicle to the lower border of the second rib, the respiration was harsh at this spot in ordinary breathing, and on deep inspiration, after coughing, there was some crepitation. Pulse 90, temp. $98^{\circ}8$. To take ten minims of equal parts of chaulmoogra and almond oil three times a day in milk—also a tablespoonful of the nitro-hydrochloric acid mixture three times a day (Brompton Hosp. Pharmacop.). After taking the oil for fourteen days, during which time he had not improved in any way and the cough had become more troublesome, he left London for Hastings; the result being therefore wholly negative.

Case 2.—This also seemed a suitable case for testing this remedy, as the disease was recent and limited. J. R., a youth 20 years of age, of strumous aspect, had been complaining for two or three months of cough, with frothy expectoration, dyspnoea, emaciation, night sweats, and loss of appetite. He had always had good health previously, and his parents and his brothers and sisters were all healthy. Pulse very quick, 140, temp. 99° . Tongue coated. Dulness over right apex to third rib, tubular respiration with crepitant râles over corresponding part of lung in front and behind. Ordered ten minims of a mixture of equal parts of chaulmoogra oil and almond oil three times a day, and a tablespoonful of nitro-hydrochloric acid mixture. Iodine to be applied over the affected area. At the end of the first week he said he was worse, the cough was more troublesome, so that he got no sleep; his appetite was very bad, and for the last three or four days

he had suffered from pains in the abdomen, and diarrhoea—the bowels acting three and four times a day. To continue the oil, but to have also *mist. sodæ bicarb. c. sp. chloroformi* ℥j; with ℥ 20 of *tinct. cardamomi comp.*, three times a day, and to have *trochisci ipecac. c. opio* occasionally for the cough. This patient after taking the medicine for a month without any improvement ceased to attend; but a report was sent to the hospital three weeks afterwards to say that he was dead.

Case 3 was a more advanced one. B. B., a dark complexioned, anæmic-looking girl of fifteen years of age, had been out of health for nearly a year; she complained of slight cough, dyspnoea on exertion, loss of flesh and night sweats; the catamenia had been arrested for twelve months. She came of a healthy family. Pulse 140, temp. 100°. Tongue clean. On the right side there was dulness over the whole of the upper lobe, with largish moist crepitant râles both in front and behind. Chaulmoogra oil was ordered as in the two preceding cases, with ℥j of the *mist. quiniæ c. acid nitro-hydroch.* (Brn. H. P.) three times a day, and iodine was applied locally. This patient complained of great difficulty in taking the oil on account of its *rising* and its unpleasant taste; she was however urged to continue it, and professed to have done so for more than two months, at the end of which time she was rather worse than better. Mr. Henderson of Rickmansworth, under whose care she has subsequently been, writes to me: “She became so much worse that I had to keep her in bed the greater part of the severe weather. The oil seemed to produce so much sickness that I was compelled to alter the treatment.”

Case 4.—Considerable improvement attended the use of the oil in this case. T. B., a clerk 17 years of age, with a good family history, had been ailing for six months with cough and expectoration night and morning, loss of flesh, and night sweats. Pulse and temp. normal, tongue coated. Right side resonance diminished all over the front of the chest, with feeble breath, sound crepitation on coughing, some depression of the chest wall over the apex of the lung. Ordered fifteen drops of a mixture of equal parts of chaulmoogra and almond oil three times a day; a tablespoonful of the *mixture ferri. c. sp. chloroformi* (Brn. H. P.) three times a day, and an embrocation of turpentine and iodine to be rubbed in over the right lung.

At the end of the first fourteen days the patient reported that he felt stronger, that his cough was better, the night sweats were better, and the appetite was better. At the end of the second fortnight he said he felt still stronger, his appetite was improved, and the night sweats had disappeared. By the end of the eighth week he reported himself as being much stronger, there had been no return of the night sweats, he was gaining

flesh, and his appetite was very good. On re-examination of the chest the resonance over the right side had much improved, no moist or crepitant râles could be heard anywhere, but the respiratory sounds over the upper lobe were rather harsh. He left for New Zealand a few days after.

It is impossible in this case to lay much stress on the efficacy of the chaulmoogra oil, as it may be argued that, after all, this may not have been a case of phthisis, but one of somewhat chronic congestion of the lung with pleuritic adhesions about the apex; hence the diffused loss of resonance, the faint crepitant râles on cough, and the retraction of the chest wall over the upper part of the lung; and the chief agents in producing the great amelioration that ensued may have been the tonic iron mixture and counter-irritation which were employed.

Case 5.—This was a very hopeless case, but I had been asked to give the oil a trial in advanced cases as well as in early ones, and so I ordered it for this patient. C. T., a girl 13 years of age, had been suffering for twelve months from cough, frothy expectoration, dyspnoea, loss of flesh, and occasional attacks of diarrhoea. Her grandfather, her mother, and an aunt had died of phthisis. She had a very rapid pulse, and a temp. of $103^{\circ}6$. There was dulness all over the right side in front, fluid crepitant râles all over the lung, and cavernous sounds in the upper lobe; on the left side scattered crepitant râles were heard here and there. To take \mathfrak{M} 15 of the mixed oils (as above) three times a day; and to have an ounce of the misturæ sodæ bicarb. c. sp. chloroformi with one minim of tincture of opium three times a day. This patient did not attend again, and died in a few weeks.

Case 6.—This was an “average” case of phthisis as we meet with it amongst hospital out-patients. J. E., aged 29, a tailor, had been ailing for five months, and suffering from cough, frothy expectoration, loss of appetite and strength, emaciation, night sweats, and occasional diarrhoea. Pulse 110, temp. 101° . Tongue denuded in patches. Right side, resonance diminished over the upper lobe, feeble breath sound, crepitations on cough and whispering pectoriloquy. Left normal. To have twenty minims of the mixed oils three times a day, and a tablespoonful of the mixture bismuth c. gentium (Br. Ph.) an hour before meals. Ipecacuanha lozenge when the cough was very troublesome. At the end of a fortnight his appetite was better, but the cough was “very bad” and the night sweats no better. He complained much of the taste of the chaulmoogra oil, and asked if he might not have cod-liver oil instead. I then ordered him the chaulmoogra oil mixed with cod-liver oil. He took this for a month, and at the end of that time he reported himself as certainly “not so well,” his cough

was worse, he was very feverish, thirsty, sweating much at night: temperature 104° . Physical signs the same as at first. The chaulmoogra oil was discontinued. He was ordered *mistura calcis hypophosphitis* three times a day, *ol. morrhuae* 3 ii. twice a day. After eight or nine weeks of this treatment he stated he felt stronger, the cough was better, and the night sweats had disappeared. Temperature $101^{\circ}8$. The physical signs had improved, the dulness had diminished, and there was less crepitation on coughing.

Case 7.—C. P., a porter, 29 years of age, had suffered from cough for twelve years. Latterly he had been much worse and complained of considerable dyspnoea, loss of flesh, and night sweats. The chest was bulged anteriorly to a great extent, and there were other signs of emphysema, feeble breath sound generally and little or no inspiratory expansion of the chest walls. But in addition there was dulness over the upper lobe on the left side, and fine metallic crepitation. He was ordered twenty minims of the mixture of chaulmoogra and almond oil three times a day; an ounce of the *mistura ferri. c. sp. chloroformi* three times a day, and the chest to be rubbed every night with the *lin. terebinthinæ*. This patient, who could not take cod-liver oil, took this mixture of oils without any difficulty. After the first fortnight he reported himself as better and stronger, the night sweats were better, but he was coughing much at night. To take an *ipecacuanha* lozenge when the cough is troublesome. At the end of a month he said he was still better, he felt stronger, the cough was better, and the night sweats were nearly gone. He did not attend again—but on inquiry it was ascertained that he was unable to get to the hospital and died about six weeks after his last attendance.

Case 8.—T. T., a young man 23 years of age, had frequently suffered from winter coughs; for the last three months he had been coughing constantly and had occasionally brought up small quantities of blood. He complained also of much dyspnoea, of vomiting in the morning, of emaciation and night sweats. He had lost both mother and father of phthisis. Pulse 110, temperature 100° . On the left side of the chest there was dulness over the whole of the upper lobe with crepitation, and on coughing loud, gurgling râles were heard at the apex. He was ordered fifteen minims of chaulmoogra oil in two teaspoonfuls of cod-liver oil twice a day; an ounce of *mistura gentian cum ammonia* (Br. P.) an hour before meals three times a day and iodine locally to the chest. At the end of a month there was no improvement. At the end of the second month he appeared no better in any respect, he thought he was worse, he had a return of hæmoptysis, and he protested that he could not continue to take the oil as it made him sick. It was therefore discontinued.

Case 9.—W. C., a clerk 20 years of age, had been suffering for three months from cough, with slight expectoration, dyspnoea, loss of flesh and weakness. Pulse 100, temperature $99^{\circ}4$. Right side dulness over upper lobe, with tubular respiration, and fine crepitation back and front. On the left side, faint sub-crepitant râles in the superior vertebro-scapular region. He could not take cod-liver oil, so was ordered twenty minims of the mixed chaulmoogra and almond oil three times a day, &c. This patient did not attend again; but sent a report to the effect that he was “not so well.”

Of the nine cases then in which I have prescribed this oil since last September—three are dead; one removed to Hastings after taking the oil for a fortnight without any improvement; one found it impossible to continue it on account of the gastric disturbance it produced; one appeared to get decidedly worse during the administration of the oil, became very feverish, his temperature going up to 104° ; then on the discontinuance of the oil and the substitution of hypophosphite of lime and cod-liver oil he mended considerably; one after taking the oil for two months was no better, and was allowed to discontinue it, as he complained that it made him sick; one discontinued his attendance after the first fortnight and sent word that he was “not so well”; and one improved considerably.

It is impossible to regard this as a favourable report of the remedial influence of chaulmoogra oil in phthisis. In only one case was there any real improvement, and although that case presented most of the features of phthisis, another explanation of the symptoms was possible, as I have shown, and the improvement may have been due to the strong iron tonic and the counter-irritation which was employed. Nor can it be fairly objected that I submitted the remedy to too severe a test in the cases I selected for its use: they were by no means worse than the average cases of phthisis as they first come under observation amongst hospital out-patients; and they were such cases as are constantly found to be greatly benefited by other remedies. At any rate, I fear it must be concluded that this drug gives no promise of help in well-marked cases, where the disease has reached the stage of infiltration and softening of any considerable portion of either lung. It requires something more than courage to persevere in prescribing a medicine which patients protest they dislike and which one is not in a position to assure them is certainly likely to benefit them; I propose however to give it one or two more trials in the form of *perles*, and in as favourable and early cases as come within my observation.—*Practitioner*, April 1879, p. 241.

36.—TREATMENT OF HOOPING-COUGH BY ATROPIA.

By Dr. ARTHUR WIGLESWORTH, Liverpool.

There are few diseases that have such a host of remedies assigned to them as hooping-cough. Scarcely a year passes but some new remedy is vaunted as a "specific," has a brief life of notoriety, and then passes into oblivion. Yet if each remedy is classed, we find that those medicines that are termed "neurotics" have the larger number of advocates, and live as it were the longest life. And this is what we should *à priori* expect when we remember that the disease is essentially a "neurosis;" yet having said this, we have said pretty nearly all that we can concerning its etiology. We know not how it originates, nor how it is propagated; and concerning the causes of its oftentimes erratic career we are still in ignorance. The fact, however, is patent, that it is one of the most uncertain of all infectious diseases. Beginning mildly, it will terminate in death; commencing with severity, it will suddenly become benign; and when to all appearance its course has ended, it suddenly reappears, after an interval of weeks or even months. Its very advent is uncertain; sometimes preceded by severe catarrh, yet often the characteristic "hoop" being the first circumstance to arouse attention. No wonder, then, that a host of remedies have been tried, and with varying success. Yet no remedy can be worthy of the name "specific," unless, indeed, under the various aspects that the disease presents, certain definite curative results follow its administration—results so certain that they can be safely predicted—results that the most unlearned cannot fail to perceive.

Now, without at the present time claiming for atropia the name "specific" for hooping-cough, I am nevertheless constrained to say that in my hands it has advanced nearer to that position than any other remedy that I have either used or seen administered by others; and that so far it has been an unexpected and unequivocal success. I was led to try atropia by reading Trousseau's remarks concerning the oftentimes beneficial results obtained from belladonna; for it appeared to me that if the experimental treatment was to be carefully pursued, more reliable preparations than the extract or the powder of belladonna ought to be used. And the alkaloid seemed to be more likely to prove useful—first, because of its unvarying strength; secondly, because the dose could be the more easily regulated in consequence; thirdly, because it is nearly tasteless. I therefore chose the solution of the sulphate of atropia (B.P.), which contains 1-120th of a grain in each minim—a most convenient strength; and in every case directed that it should be administered in the morning fasting. Also, whenever it could

possibly be accomplished, I directed that the number of paroxysms should be carefully noted from 8 a.m. to 8 p.m. In this I departed from Trousseau's recommendation that the number of paroxysms should be noted for twenty-four hours; because it is practically useless to attempt to get trustworthy statistics during the night except in those very severe cases where the attendance of a night-nurse is imperative; and, further, if we find that there is either an increase or a diminution during the day, we may feel quite sure that the same ratio will exist during the night.

I commenced, then, over four years ago to treat all cases of whooping-cough solely with the solution of sulphate of atropia, from infants two months old to the adult. It required some little time to find out the average dose to begin with; but I now begin with 1-120th of a grain (or one minim in a drachm of water), in children from one to four years of age, either diminishing or increasing the dose as occasion dictates; and, except in very severe cases, only order it to be given once a day; but when the nightly paroxysms are very severe, I order half the dose to be repeated about an hour before bedtime.

The results that follow its administration may be summed up thus:—1st. There is a steady diminution in the *number* of paroxysms. 2nd. There is a diminution in the *duration* of the paroxysms. 3rd. There is a change in the character of the "hoop," as if the vocal cords were not so closely approximated. Further, if the atropine is withheld the beneficial effects derived from it subside.

Now these results follow more or less speedily the administration of the remedy, and appear to depend upon the susceptibility of the patient to the action of atropia. In a few cases thirst may become a prominent symptom, which subsides, however, upon a diminution of the dose. In only one case has the sensation of "falling down" been experienced, and this disappeared with a reduction in quantity.

I append a few cases, taken at random from details before me. In one the remedy was commenced as soon as the characteristic hoop was definitely established, and the daily number of paroxysms ran up from eleven to twenty-six in four days; by that time the atropia seemed to have begun to act, and in fourteen days the number was reduced to two. In another case the number was twenty-four on the second day after the atropia was commenced, and a similar decline took place. Again, twenty-six was the number first registered; in three days it had fallen to thirteen, remained stationary for a week, then steadily declined. In another patient, aged ten years, the cough was reported as incessant; in a week there were no paroxysms to record. One case I should like to give more in detail. The

patient, a little girl, aged five, had passed through a severe catarrhal stage, and the "hoop" had become well-established, when she had a sharp attack of measles, but no diminution of the cough. The rash was nearly gone when acute bronchopneumonia of both lungs set in, dulness on the left side being well marked half-way up the lung, respirations seventy-five in the minute, dyspnoea urgent, recumbent position impossible, pulse 170, fluids could only be partaken of in single mouthfuls for "want of breath." Each pertussal paroxysm caused the greatest distress. In this case I ordered 1-120th of a grain of sulphate of atropia *twice* a day with the most satisfactory result. The cough became less frequent, and its duration was decidedly shortened and less violent. She was also ordered in the interval carbonate of ammonia and decoction of senega. Under this treatment the child slowly rallied, and is now perfectly well. Here the gravest and most imminent danger existed, and I doubt whether it was possible for the child to have lived had not the paroxysms of whooping-cough been curtailed in frequency and duration. It is worthy of record that a near relative who was in close attendance upon the child was called away for forty-eight hours, and upon her return spontaneously remarked "that the character of the whooping-cough seemed to have become quite different during her absence." A good example recently occurred in the family of a clergyman, three of the children being attacked with whooping-cough coincidently with their neighbour's family with whom they played every day. When I first saw them the cough was well established, severe in character, with constant vomiting. In a month all trace of the disease had left them, whilst at the end of two months their neighbour's children still had it.

Such briefly has been my experience of the value of atropine. When the complete cure was not speedy, the diminution in the paroxysms was so marked, and so rapidly was the affection alleviated, that the children seemed not to be at all inconvenienced. I should say that in by far the majority of cases the "hoop" had subsided in a month, sometimes in three weeks. In only one case did the remedy seem ineffectual, and that was a child about seven years of age, brought to me from some distance. I had doubts about the remedy being properly administered and persevered in. In three weeks it was reported no better. Six months after the cough was stated to be just the same, minus the "hoop." A close examination of the chest failed to detect any abnormal influence at work. I was inclined to the idea that there was a little hysteria, the child's health continuing perfectly well and appetite good, and yet the cough was reported as incessant, and preventing sleep. This was the only instance in which decided and almost immediate relief was not obtained.

A few words as to the *modus operandi* of the drug. Hooping-cough is essentially a "neurosis," and if we are to judge from the sensations described to us by those who are old enough to analyse their feelings, it is the laryngeal branches of the pneumogastric nerve that are primarily affected. The result of this affection is that at intervals a series of reflex phenomena present themselves, varying in duration and intensity, which involve nearly all the branches of the pneumogastric. The frequency, however, of the paroxysm is no index to its severity, and conversely; nevertheless the frequency and intensity of the phenomena exhibited may be, and often are, coincident. Is there any explanation to be afforded of this? I think so. No one who has much to do with the ailments of childhood fails to observe what very different effects are produced by apparently the same irritation. In one there are excited convulsive movements, fever, restlessness, &c.; whilst in another, perhaps of the same family, there is scarcely any systemic disturbance produced. This is probably due to the inherent susceptibility of some children to "sympathetic" action and reflex phenomena; and as nervous exaltation, or "nerve tension," is far higher in children than adults, we see, as a rule, greater severity in the paroxysms of youthful than adult life, and—according to idiosyncrasy—greater severity and consequent prostration in some children than in others.

Concerning reflex phenomena, Schroeder van der Kolk has shown that the medulla oblongata is the principal centre whence the more general reflex movements derive their origin, having a special capacity for exciting them; further, that, even in a healthy state, but in consequence of augmented irritation, this capacity is greatly increased, and it will sometimes rise so high that reflex phenomena will manifest themselves spontaneously, without further eccentric irritation. We can therefore understand how the peculiar irritation (which is probably excited at the periphery of those portions of the laryngeal nerves which supply the larynx and trachea), having been transmitted to the medulla oblongata, calls forth those reflex actions which in their entirety produce the phenomena of hooping-cough; and, further, if this irritation is constantly propagated, either through the action of a morbidly excited sympathetic nerve or from idiosyncrasy, the medulla oblongata may at length become so sensitive, of such exalted capacity, that severe reflex phenomena, even to the extent of convulsive action, may spontaneously arise. And it is probable that in all cases of hooping-cough the degree of excitability capable of being produced in the medulla oblongata is the explanation of the various degrees of intensity in the reflex phenomena which we are so frequently called upon to witness; or, in other words, the intensity and

frequency of the paroxysms of whooping-cough are in direct ratio to the amount of excitability to which the medulla oblongata is inherently liable. Hence the treatment of whooping-cough should be directed, primarily, to diminishing the capacity for reflex excitability, either eccentric or centric.

Of all drugs there are none that have such a peculiar and special effect upon the pneumogastric nerve as belladonna, though it is by no means limited to that nerve. It is essentially a nervine sedative, and has a capacity for diminishing both sensibility and irritability when these are morbidly increased. Its primary effects are manifested upon the mouth and throat, producing thirst. A further action is upon the laryngeal muscles, rendering articulation imperfect, or preventing it altogether. So also upon the constrictors of the pharynx, that deglutition becomes difficult or impossible. These and other effects are produced more or less according to the amount taken. It is reasonable, then, to attribute the beneficial effects of atropia in whooping-cough chiefly to its effect upon the laryngeal branches of the pneumogastric nerve, diminishing the exalted sensibility and irritation which are known to exist, and which, by constant propagation to the medulla oblongata, increase in that body the capacity for reflex phenomena. But it is also probable that atropia has a very decided effect upon the medulla oblongata itself, rendering it less capable of exciting reflex action. Dr. Kroon's experiments led him to the conclusion that valerianate of atropia had a very special and direct effect upon it, diminishing its inherent capacity for reflex phenomena. The almost specific effect of belladonna in preventing nocturnal seminal emissions is also probably due to this action.

I think, then, the conclusion is justified that, by its action upon the pneumogastric and sympathetic nerves, and also upon the medulla oblongata, atropia relieves, and ultimately cures, the neurosis called whooping-cough; and that in those cases where, from idiosyncrasy or easily-excited sympathetic action, the intensity and severity of the reflex phenomena are greatest, the beneficial action of atropia will be more marked.—*Lancet*, April 12, 1879, p. 513.

37.—ON THE TREATMENT OF CROUP.

By Dr. JOHN MOIR, District Medical Officer, West Ham Union, London.

It is by no means unnecessary to mention that there is no *specific* remedy for croup, that is, in the ordinary acceptation of the term as it is used by quacks and impostors, and as apprehended by the ignorant, in matters of this sort, the vast majority

of the nation. Yet, as I hope to be able to show, we have remedies which remove this undoubtedly serious malady from a scale it once occupied, so low, that in comparatively recent times nearly one-half of the cases attacked terminated fatally, to a much more tractable class of disease, whereby, if the case is attended to sufficiently early, the mortality may be reduced to the smallest minimum. It may be truly said here, however, and reiterated again and again, that in most cases everything depends on seeing the case in its very earliest stage. I will illustrate this by quoting a case under the treatment of Mr. Meade, of the Bradford Infirmary, communicated to the *Lancet* in July, 1871, by Dr. Andrea Rabagliati. The patient was a boy of six years, who died in nineteen hours, after many severe fractures from the fall of a flagstone on him. On post-mortem examination an abundant thick deposit of organised lymph was found over nearly the whole surface of the brain. Dr. Rabagliati hereupon remarks—"There having been no inflammatory symptoms of any consequence during life, it seems extraordinary that so extensive an exudation could be poured out and organised in so short a space of time as nineteen hours. From a series of careful observations made on pus-formation in this infirmary, it seems that the existence of true pus-cells cannot be satisfactorily determined by the microscope under at least thirty-six hours after the infliction of the injury. This appears to hold good as well for wounds made by the surgeon (as in amputation) as for lacerations of the soft tissues caused by injury. Such observations afford important data for estimating the comparative rapidity of formation of two common products of inflammation. It may be the case that the extremely rapid pouring out of the exudation in this case was due to the active condition of the vital powers in childhood; but if this be so, it tends to enforce the necessity of *prompt and efficient treatment* in those diseases which, like *croup* and *diphtheria*, depend on the rapid formation of inflammatory products."

It is impossible to mention here every remedy that has been used with more or less advantage in the treatment of croup, we will therefore confine our attention to those undoubtedly of the greatest value in the successful attainment of this object. This leads, in the first place, to the consideration of the prophylactic measures, if any, which ought to be resorted to, and here, at least, it must be confessed, that we have no special remedy to offer beyond those advisable for the prevention of other ordinary catarrhal and respiratory affections.

Prophylactic Treatment of Croup.—As croup is generally induced by dampness, cold, and moisture, and is therefore more common among poor children than those of the wealthier classes, although it often attacks the robust and well-nourished

also, it is difficult to put into execution the chief hygienic prophylactic measure we possess, that is, removal from the locality where the affection is common to some other where it is unknown. It is also very important, where it can be effected, to select a dry position for the dwelling-house, such as the side of a hill with a southern exposure, and cellared underneath; such an arrangement would secure warmth in winter and coolness in summer, and in very severe weather confinement to the house, though tedious, might yet be alleviated by the enjoyment of light and sun, and a mild artificial and dry temperature. But this is only attainable by the few, so that we must see if there are not more general means of avoiding these attacks.

It is advisable that children with croup should be isolated as far as possible from other children unaffected by it, inasmuch as it is probably too much to say that it does not occasionally become epidemic, and children of the same family are sometimes more likely to take it than others. Dr. George Johnson, at least in this country, adopts this principle. Avoiding damp, cold, and moisture as much as possible, with the other usual precautions against wet feet, &c., which have so large a share in forming the groundwork of future robustness, and preventing or modifying many of the maladies incident to infancy and childhood, are useful here. Warm clothing thoroughly protecting the body, especially the respiratory apparatus; flannels next to the skin, woollen stockings and substantial boots, cleanliness, and the frequent use of the sponge-bath, the water of which in the case of young children should never be entirely cold. The child should never be allowed to run about the room after being taken out of the bath, but should be at once thoroughly dried and put into a warm bed, with a sufficient quantity of bedclothes, especially blankets. The temperature of the bedroom ought to be at least equal to that of the room occupied during the day.

Inunction with oil, the application of olive or salad oil to the skin, is probably the most beneficial of all measures for the preservation of health and the strengthening of body in delicate children, not only to prevent an attack of croup, but in scrofula and consumption and other diseases more or less akin, and often complicated with the scrofulous diathesis, such as rickets, tabes, laryngismus, tinea, impetigo, &c., it is invaluable. In inanition from whatever cause, especially when produced by mal-assimilation or mal-nutrition, and combined with a dry and disordered state of skin, this practice of oil-inunction is of the utmost advantage. Sir James Y. Simpson says,—“In the marasmus of children, I have more than once seen oil-inunction succeed and apparently save life when all other means and remedies had utterly failed. When the body is much reduced by morbid

eliminations, or by acute or chronic disease,—as after the dysentery and diarrhoea of children,—oil-inunction sometimes forms the best restorative. In rheumatism and in the chorea of the young, when accompanied by debility, it is often serviceable.” The sum of Simpson’s observations is, that the practice guards weak constitutions against the effects of changes of weather and temperature, and the feeling of cold, and tendency to catarrh and chilliness, attendant upon various debilitated states, is sometimes entirely arrested and averted by oil-inunction; for it is the property of oils, as Pliny long ago remarked, “*Tepefacere corpus et contra algores munire.*” The external, like the internal use of oil, also, is apparently more efficacious in children than in adults, the absorbing power of the skin being much greater in youth. The mode of using the oil will vary according to circumstances; but, generally speaking, a wineglassful, slightly warmed so that it may be more easily absorbed, should be applied every night at bedtime over the whole cutaneous surface of the trunk and extremities, especially where it will be most readily absorbed, between the thighs, the flexures of the limbs, and the sides of the body. For obvious reasons the night-dress should be of flannel extending well up round the neck, and down beyond the feet, although children’s night-dresses might always advantageously be of this material. To further promote absorption warm sponging of the body might at first be employed, but after the first two or three weeks this is hardly necessary, as less time and friction are required the longer the practice is kept up. The oil should always be rubbed in till the process of absorption is complete, requiring at first from fifteen to twenty minutes; afterwards from ten to fifteen minutes is all that is usually required. If at first a little unpleasant, or so supposed—for the supposition is purely fanciful—it soon becomes the reverse, and greatly promotes both cleanliness and sleep; as with the bath, so with oil-inunction, many find it difficult to give up after getting accustomed to it. It is used by some Asiatic nations at this day as a great luxury, as well as a means of strengthening and refreshing the body. The Bible contains allusions to the practice among the Jews, and it formed part of the daily life of the ancient Greeks and Romans, both as a habit and luxury. Seneca particularly notices it, and Plato remarks, in his *Historia Natur.*, book xxiii. chap. 22, “*Duo sunt liquores corporibus humanis gratissimi; intus vini, foris olei.*” I can state that, in many cases of children in which I have adopted this treatment, it has been followed by the most gratifying results.

Cod-liver oil internally, tonics, wine, milk, and careful attention to diet, &c., are also measures that may be useful as prophylactics under certain circumstances, both in this and other diseases.

But, as we have observed, croup attacks all classes and every manner and habit of life, and such being the case, notwithstanding the diligent use of any or all of the precautionary measures above mentioned, as in the neglect of them, an attack of the disease may set in. What, then, are the remedies to be used when an attack of croup has actually commenced? This leads us to consider, firstly, the medical remedies as distinguished from the surgical, which are sometimes necessary in a later stage of the complaint.

Medical Treatment of Croup.—Here also we find that diphtheria differs from croup, for diphtheria must be regarded as a constitutional disease requiring constitutional treatment; croup requires such also, but of a very different kind. The after-treatment of diphtheria and its sequelæ is frequently of vital importance; croup, on the other hand, as a general rule, requires no after-treatment.

In diphtheria, iron, quinine, wine, bark, chlorate of potash, are the chief remedies, with local application of the saturated solution of the perchloride of iron, with glycerine, chlorine, Condyl's fluid, carbolic acid, &c., are used. In croup, emetics are the principal remedies; and local applications, although they have been tried and with success, can hardly be included amongst the remedial measures to be employed, both from the difficulty of their application, and the danger incident to them on account of the proximity of the bronchi. Mr. G. Hamilton, in the *Edin. Med. Journal* for 1863, gives a good description of topical applications, and their mode of use, the latter being particularly interesting and instructive. M. Brétonneau, also, in his *Fifth Memoir* (to Dr. Blache), 1855, states that he had found it efficacious, notably where the divisions of the bronchi were already affected, and surgical treatment, tracheotomy, entirely out of the question. The remedy he employed was nitrate of silver, and in one case about an ounce in strongly saturated solution on a piece of sponge was employed for four consecutive days, eight applications being made daily. As in several cases recorded by Mr. Green, of New York, and various others, this heroic treatment was happily crowned with complete success. Still, topical applications are not to any extent the remedies we must chiefly rely on.

What we hope for, as I have before noticed in quoting Dr. Rabagliati's case, is beyond everything to get the disease in its earliest and most limited form, while still confined to the trachea and larynx; this granted, the two great points to be attended to are the limitation of the disease to these parts alone, and the expulsion of the false membranes and prevention of their re-formation. Neither prophylactic nor local measures being now applicable to this state of matters, or the last in so

few cases as to be practically put out of court, what must we trust to ?

Blood-letting, calomel, and antimony may in suitable cases occasionally prove useful, very particularly where the patient is robust, and the croup is complicated with any other inflammatory disease, as pneumonia or pleurisy ; but I am doubtful of their utility even then, and we must not forget that in cases where our patient is very feeble, and shows only very faint signs of reaction, we might cause or at least hasten death by using any of these means, however beneficial they might be under other circumstances. They should never be used in any case except at the very beginning of the disease ; then, if at this very early stage suffocation seems imminent, and inflammation complicating or threatening extension of the disease, I have seen a leech applied to the top of the sternum produce very great benefit, and small doses of calomel with sugar, or Bouchut's recipe, for this stage of croup, Antim. tart., gr. 4-8 ; Syr. simpl., ℥ss. ; Mucilag. ad. ℥iij. Sign. One teaspoonful every hour. But although good results have been obtained from them, as in some of M. Brétonneau's cases, and in those of M. Notta, recorded in the Brit. and Foreign Med. Chir. Review, 1865, I very much doubt if they should ever be given, and have had reason to regret administering them in some instances. Dr. T. Hawkes Tanner, Dr. J. Hughes Bennett, &c., are entirely, and with good reason, against the "lowering treatment, considering antimony and tartar-emetic and calomel worse than useless in croup, pneumonia, &c. They recommend wine or brandy, liberal diet, sedatives, and rest ; and all treatment as subordinate to fresh air, proper food, free exercise, washing and oiling of the body, &c., as *preventatives* of disease. I fear that, in some cases at least, valuable time is lost also by the calomel or antimonial treatment ; and as in this disease more than in any other we may say, *bis dat, qui cito dat*. Above and beyond all other remedies, I would place emetics to carry out the two points of treatment before mentioned, limitation of the disease, and expulsion of the false membrane, and to prevent their re-formation—Dr. Tanner's or Dr. Richardson's treatment, to be presently mentioned.

This being the case, the hot-bath about 96° F., poultices of hot oatmeal or linseed-meal, and mustard with emetics, are especially to be recommended. While the disease is still early, nauseating emetics may be employed ; the best of these are ipecacuanha and antimony, either alone or in combination, but certainly in the more advanced stage, if not from the first ; and in future I intend using from the very first appearance of croup, as I have done lately, those which do not depress, such as sulphate of zinc, and more particularly, as the best remedy in croup, sulphate of copper should be used.

Long ago used by Hippocrates and his followers as *mel cup-ratum*, still one of the formulæ of the French Codex, and most recently very extensively and successfully used by Dr. Crichton of Tavistock, sulphate of copper, according to Hönerkopf, Sainter, Luzsinsky, Serlo de Crossen, Dr. Hoffman, and M. Daviot, is the most valuable agent we possess in the successful treatment of membranous croup. Its only objection, when not carefully administered, sometimes even it must be admitted, when most carefully administered, is a tendency to produce violent diarrhoea, from its action on the gastro-intestinal mucous membrane. But if employed, as it always ought to be, in half-grain doses—never, at least, in more than grain doses every ten minutes—until free and repeated vomiting has been produced, it is the most reliable of all the remedies in the cure of croup. Of fifty cases treated by sulphate of copper, in half-grain, or not more than grain doses, every ten minutes until free and repeated vomiting was produced, by Dr. Crichton, all under eight years of age, and suffering from genuine croup, only six died, one after scarlatina, two during an attack of diphtheria.—*Edinburgh Medical Journal*, March 1879, p. 796.

38.—ON THE TREATMENT OF ASTHMA.

By Dr. JOHN C. THOROWGOOD, Physician to Victoria-Park Hospital for Diseases of the Chest, and to the West London Hospital, &c.

The capriciousness of bronchial asthma with regard to external influences is well exemplified by the effect of climate on the complaint. To many asthmatics the pure clear air of such places as Margate, Whitby, or Nice, is absolutely unbearable; and we find patients hurrying away from such climates, almost in terror of their lives if another night be passed therein. Dr. Salter has gone into the history of twenty-one cases of asthma powerfully influenced by atmospheric surroundings, and the outcome of evidence is conflicting and unpractical. Fourteen could respire with comfort only in the smoky air of populous cities; eleven, indeed, seemed cured by the air of London. Seven others were unable to live in close towns, and did best in the country. One patient of my own, a great sufferer from asthma at his house in the country, passed six weeks in Queen Anne Street, Cavendish Square, without experiencing a single attack of breath spasm. On returning home he was at once taken in the night with so alarming a seizure that he had to send urgently for his regular medical adviser. Another patient was glad to quit his well-furnished abode in the northern outskirts of London and take up his residence at his business premises in Cornhill, for there he could rest at night without fear of

an attack of asthma. Those who have read Walshe's work on the Lungs will remember the instance of the man, sorely tormented with incurable asthma near Hampstead, who was absolutely cured by changing his residence to the more central region of Seven Dials. I once thought it might be the absence of ozone in the air of cities that renders it sedative to irritable air-tubes; but though experiment and observation clearly show that ozone produces catarrh, and even pulmonary inflammation, yet bad asthmatics have been known to make a voyage to Australia without experiencing any approach to a seizure the whole time, though the air was shown to be often highly charged with ozone.

Among remedial agents employed during a bad paroxysm of asthma, few are more universally known and employed than the smoke of burning nitre-paper. Ordinary blotting paper is soaked in a strong solution of nitrate of potash, and, when dry, is burnt till the patient's room is filled with a cloud of nitrous fume, and often it is not till the room is thus filled with smoke that the sufferer finds relief. As long ago as 1846 we find this use of nitre-paper highly extolled in the Medical Gazette; and in the Lancet of April 5th, 1845, we learn how a friend of Mr. Harrison's tried burning paper that had been soaked in saturated solution of nitrate of potash, and felt the inhaled smoke to clear the passages and open the air-tubes, and so conduce to free and easy respiration. Though some amount of emphysema and bronchitis does not invalidate the action of the nitrous fume, yet it is in the spasmodic sudden attacks of breath stoppage where its curative action is most marked. The chemical constituents of the fume have been examined by M. Vohl, and he found the nitrous vapour to contain cyanogen, carbonic acid, nitrogen, ammonia, and nitrite of potash. To the ammonia and nitrite of potash the antispasmodic property of the fume is attributed. Noticing that the constituents of the nitrous vapour are closely allied to some of the adventitious matters found in the close air of towns, I went over Salter's table of 223 asthmatics to see if I could discover that those persons who are set down in the table as being notably relieved by the inhalation of the nitrous vapour were identical with those to whom town air proved so beneficial. I found that of twenty-one who are said to have been greatly benefited by the air of London, there were nine to whom the nitre-paper was of marked service. Two who are reported as cured by the London air, found nitre-paper inhalations more serviceable than anything else. One patient, whose sufferings were intensified in London, reports that nitre-paper seemed to do harm rather than good. Probably it is to the ammonia and carbonic-acid gas that we may attribute the antispasmodic action of the

burning paper. It has been shown by Trousseau how useful the inhalation of diluted ammonia can be in asthma, and Salter records a case where asthmatic spasm was at once stopped by breathing the vapour of diluted ammonia. The fact that the emanation from certain gas products is reputed good against the convulsion of whooping-cough, and that an asthmatical sea-captain could always breathe with facility when conveying a cargo of guano, are additional proofs of the antispasmodic action of ammoniacal vapour.

Among internal medicines that are good against spasmodic asthma, even when some degree of chronic bronchitis coexists, a high place should be given to the arseniate of soda. In a commencing dose of one-twentieth of a grain, either in pill or solution, this salt has a great power of promoting respiratory action, and appears, moreover, to act as a tonic to the system, for I have noticed patients to gain appetite and weight during its administration. The Bourboule water, so beneficial as a drink for those subject to asthma, apparently owes its efficacy to a trace of arseniate of soda in its composition. Arsenic-smoking, by means of cigarettes containing from a quarter to half a grain of arseniate, is very comforting to those who suffer from hay asthma, or from ordinary spasmodic asthma, provided no feverishness of system or inflammatory action be going on. Dr. Wilks told me some time ago of a former patient of his at Guy's Hospital, who, in his trade as a bird-stuffer, used much white arsenic. This man was asthmatical and a smoker, and, when taken with one of his breath attacks, he usually added some of his white arsenic to the contents of his pipe, and speedily found relief.

Lately the iodide of ethyl has been employed as an inhalation in the treatment of asthma. Pure iodide of ethyl is colourless, but, after a while, it is apt to acquire a brown tinge, if exposed to the light, from the liberation of free iodine. Dr. Andrew and I have used this preparation in a few cases in the Victoria Park Hospital, and with beneficial result. In one case under my care a young woman had an asthmatic seizure every morning at 4 a.m. of some severity, but was eased much by the use of strong coffee. We gave her for a week ten drops of the iodide of ethyl on lint three times a day for inhalation, and at the end of the week she had lost her cough as well as her asthmatic attacks, nor did they show any sign of returning during the remaining three weeks of her stay in the hospital. My clinical assistant, Mr. M'Donald, was able to detect iodine by the starch test in the urine and expectoration of those who were inhaling the iodide of ethyl.

In two cases recently I have observed excellent effects follow on the employment of the citrate of caffeine. One patient was

an eminent medical practitioner in a large town in the north. He had suffered most severely from paroxysmal asthma, and the utter failure of a list of approved inhalations and medicines (far too long to be here enumerated) was most distressing. Four grains of citrate of caffein produced an undue degree of wakefulness, but one grain, taken regularly at bedtime, had a most happy effect indeed. So far as we can at present judge, it appears to have been really curative of the asthma. The last report says: "*Pari passu* with the asthma my cough and expectoration have gone, and I now have next to none of either." In another case a highly informed and observant patient of Dr. Kingsford's, who had found much benefit from the inhalation of iodide of ethyl for an asthma and bronchitis of twenty years' standing, tried the citrate of caffein in two-grain doses every afternoon for a fortnight without any marked result. One day, however, being sadly worn out by a protracted attack of bronchial spasm which had lasted for eight hours, this patient took four grains of citrate of caffeine in coffee, with the effect of obtaining immediate relief to the spasm, followed by three hours' quiet sleep in his chair. The citrate of caffein appears to allay the abnormal excitability of the nerve-centres, and then repose ensues as a natural result. I have seen similar calming effect from the use of nux vomica in bad emphysematous asthma, with failing pulmonary innervation, and this soothing and soporific action of remedies reputed nerve-tonics and stimulants in cases of great exhaustion of the nerve-centres is an interesting therapeutical fact, ably set forth by Dr. Milner Fothergill in his Fothergillian prize essay. The citrate of caffein is not a new remedy. It has been employed in neuralgic headache and sickness for many years, and a friend of mine saw it given for asthma in Paris more than twenty years ago. Lately Dr. Lewis Shapter has employed the citrate as a diuretic in cardiac dropsy. Like belladonna and atropia, caffein appears to stimulate the respiratory and circulatory nerve-centres, and to increase the blood-pressure in the small vessels, so that it may prove an efficient diuretic. As such it may be employed in cardiac dropsy, where digitalis does not appear to agree. The citrate of caffein, a fine specimen of which has been sent to the Society by Messrs. Corbyn, occurs in delicate silky crystals soluble in water, but best administered in a small cup of coffee. The dose may commence with one grain and extend to four or five.

Space does not allow me to dwell further on the subject of the treatment of asthma. In old-standing emphysematous asthma it is important to relieve any abdominal plethora and congestion of liver; and when this is done by means of saline aperients, Carlsbad salts, and abstinence from all forms of

alcohol, then great benefit will often follow on the use of small doses of the extract and tincture of *nux vomica*. I have repeatedly proved the value of *nux vomica* and *strychnia* in cases of emphysematous asthma, with very prolonged expiration, and apparent tendency to paralysis of bronchial muscle. It is in cases of old emphysematous asthma that the compressed air bath is so much commended. My experience of this mode of treatment is not very great. I have known a well-arranged compressed air-bath perseveringly tried by a youthful subject of bronchial spasm, whose heart and lungs as yet manifested no sign of structural change without any beneficial result. In the case of elderly persons with overfilled lungs, long weak expiration, and tendency to obstruction of the circulation, I certainly think the bath is worth a trial. In one case of this description a free expectoration came on under the influence of the compressed air, and in another the effect of the bath was to cause tranquil and refreshing sleep. One great effect of the compressed air-bath I consider is the production of more perfect oxygenation of the blood, hence its very marked effect in curing cases of obstinate anæmia. Coupled with this we may bear in mind the fact that the inhalation of oxygen gas has been found by Dr. Cayley and others of great service in relieving the dyspnœa of chronic emphysematous bronchitis, and also that Trousseau, in treating some very severe forms of anæmia, found no remedy so powerful as the inhalation of oxygen.—*Lancet*, February 15, 1879, p. 220.

DISEASES OF THE ORGANS OF DIGESTION.

39.—ON THE DIAGNOSIS OF ABDOMINAL TUMOURS.

By CHRISTOPHER HEATH, Esq., Holme Professor of Clinical Surgery, London.

The following tumours are common to both sexes.

Ascites, or dropsy of the peritoneum, gives an uniform roundness to the lower part of the belly when the fluid is small in quantity, or general distension of the abdominal walls if much fluid be present. The skin is tense and shining, and the umbilicus flat or protruding, and the superficial veins enlarged. On palpation, a distinct wave of fluid can be felt from one side to the other; and when the patient is recumbent, the intestines float forward, giving a clearer note on percussion in front than in the loins, where the fluid collects. On turning the patient on his side, the fluid gravitates to the lower part, and a clear percussion-note may be obtained on the higher side provided the abdomen be not very tense.

In a case of moderate ascites, it will be possible to map out

the liver, stomach, and spleen by careful palpation and percussion; but if a large quantity of fluid be present, this will be impossible until paracentesis has been performed.

A *distended bladder* is in the median line, and bulges out the central portion of the abdominal wall. Percussion over it is dull, unless some coils of small intestines should happen to cover it, which is not unfrequently the case, while both flanks are clear when the patient is recumbent. Pressure over the tumour causes pain and a desire to micturate, and the use of a catheter results in its gradual disappearance.

Tympanites, or general distension of the intestines, is not unfrequent in hysterical women, in whom borborygmi, or gurglings, are commonly heard. Extreme tympanites may occur in either sex as the result of intestinal obstruction, in which case the distended coils of small intestine may be felt or seen rolling about beneath the tense abdominal wall. Or it may occur as the result of peritonitis, in which case the intestines are usually fixed. The percussion-note in all cases is tympanitic.

Solid Tumours, dull upon percussion, and to be readily mapped out, provided there be no ascites, may be connected with the liver, spleen, intestines, or kidney. A tumour occupying the right hypochondrium, and extending forwards to the middle line or beyond it, and downwards to the pelvis, dull on percussion and solid to the touch, or possibly with a fluctuating spot, will be the liver. The diagnosis will be rendered certain if the edge of the liver with the notch in it can be felt.

A tumour occupying the left hypochondrium, and extending forwards and downwards, dull on percussion, and with a notch in its border, must be the spleen.

A small hard mass, slightly changing its position from time to time, will be either a mass of fæces impacted in the intestine, or a mass of cancer attached to its wall. Impacted fæces are most common in the large intestine, and give a somewhat doughy sensation to the fingers when steadily pressed against the mass. Hard cancer is most frequent at the pylorus and the lower end of the small intestine, close to the cæcum, or in the sigmoid colon, and is perfectly unyielding.

An obscure tumour in the loin can be best examined when the patient is recumbent, one hand being placed beneath the loin, and the other immediately below the false ribs, the abdominal muscles of the patient being relaxed as far as possible by flexing the thighs, and bending forward the trunk. If it be a mass of fæces in the ascending or descending colon, it will be readily felt; but if an enlargement of the kidney, it will be more deeply placed, and the resonant colon will be found in front of it. The possible existence of a movable kidney must be borne in mind.

A *fluid tumour* in the loin must be due either to cystic degeneration of the kidney, or to psoas abscess. The kidney may, owing to obstruction of the ureter, become enormously distended with fluid, so as to form a distinctly fluctuating tumour in the loin, which never finds its way into the groin. A psoas abscess, on the other hand, tends to pass into the groin, and fluctuation may usually be traced beneath Poupart's ligament into Scarpa's triangle, where an impulse will be felt on the patient coughing. Symptoms of caries of the spine, with, probably, irregularity of the spinous processes, will be found if carefully looked for.

An obscurely fluctuating swelling in the *iliac region* will probably be an iliac abscess due to disease of the pelvis or lumbar vertebræ, or of the sacro-iliac joint. The condition of this joint is best tested by forcibly squeezing the innominate bones together, and then attempting to draw them asunder by pressure on the iliac crests.

On the right side, a fluctuating swelling in the iliac region may be due to a perityphlitic abscess, or abscess caused by inflammation of the cellular tissue around the cæcum, the acute symptoms of which will be present; and, if perforation of the cæcum have occurred, there will be crepitation of the cellular tissue from the escape of the intestinal gas.

In the *male*, a solid tumour in the iliac region may be due to a retained testicle taking on inflammatory swelling, in which case acute inflammatory symptoms will be present, or developing medullary cancer with considerable rapidity. The presence or absence of the testicle from the scrotum, which should always be investigated, will give the clue to the case.

In the *female*, the possible existence of a "phantom tumour" must not be ignored; for occasionally the irregular contraction of the abdominal muscles gives rise to a tumour of such solidity as to deceive the most experienced surgeon, but disappears absolutely under the influence of chloroform. No doubt some of these phantoms have been examples of loose kidney, in which the organ is readily displaced.

A tumour in the median line, rising out of the pelvis, is probably uterine, if it be not the distended bladder. Pregnancy is first to be eliminated by inquiry as to menstruation, by examination of the breasts, and by listening for the foetal heart, which, after the fourth month, ought to be recognisable. Lastly, a vaginal examination will determine whether the *os uteri* is soft and velvety, as is the case in pregnancy. All suspicion of pregnancy being removed, the introduction of the uterine sound will determine whether the long diameter of the uterus is greater than the average (two inches and a half). Supposing the uterine sound to pass four or five inches readily, and to

move with the tumour when it is pressed from side to side, it is obvious that the tumour is uterine, and probably a fibroid.

A tumour occupying one side of the abdomen, having grown up from the pelvis, is probably ovarian. It is dull on percussion and elastic to the touch, or if of large size, may fluctuate distinctly. If no ascites be present, both flanks will be resonant, in whatever position the patient is placed; but, if there be fluid in the peritoneum, the most dependent part will be dull, though the dulness over the tumour will not vary.

When fluctuation is present, but it is doubtful whether it is ascitic or ovarian, an assistant's hand pressed edgeways into the median line over the tumour will serve to break the wave of ascites and thus clear up the doubt.

A cyst with such thin walls that the fluctuation closely resembles that of ascites is probably a cyst of the broad ligament (parovarian); and tapping will make its nature evident at once, the fluid being perfectly limpid, whilst that of ascites is yellow serum, and that of an ovarian cyst darker and, as a rule, more viscid.—*British Medical Journal*, March 1, 1879, p. 299.

40.—ON THE USE OF CITRATE OF CAFFEIN AS A DIURETIC IN CARDIAC DROPSY.

By Dr. LEWIS SHAPTER, Physician to the Devon and Exeter Hospitals.

Caffein, ($C_8H_{10}N_4O_2$), which exists in the form of silky prisms soluble in about sixty parts of water, is a principle contained in coffee, tea, guarana, and other plants used in different countries to form stimulating or restorative beverages. In the form of a soluble citrate it has been used in doses of a grain in tic-douloureux, hemicrania, and varied forms of neuralgia, but with doubtful success. It is to Professor Gubler (*Le Progrès Méd.*) that we are indebted for drawing attention to the special property of the drug in the form of citrate or bromhydrate in doses of from iv. to viij. grains, as inducing abundant and instantaneous diuresis in cases of cardiac dropsy, either when given hypodermically or by the mouth. The object of this paper is to confirm this observation on the diuretic property of the citrate of caffein, and to seek for some physiological explanation of its use; but as the drug has a manifest tendency to induce nausea and vomiting, the dose has in each case recorded been limited to gr. iij. under which copious diuresis has generally been established even in the most advanced cases of cardiac dropsy.

Case 1.—Wm. W., aged 45. Admitted into the Exeter Hospital, July 25, 1878. On admission there was urgent dyspnœa with the maintenance of the sitting posture; general

anasarca, but specially of the lower extremities; slight ascites, and the quantity of urine passed per diem varying from a pint to a pint and a half, although free from albumen. Examination revealed an enfeebled jerking rapid pulse-stroke, a tumultuous irregular cardiac action, a systolic murmur conveyed to the axilla at the heart's apex, a diastolic at base, together with crepitation and slight dulness on percussion at the bases of both lungs—or the physical signs of mitral and aortic insufficiency with congestion of the lungs. This patient was at first placed on a mixture containing tinct. digitalis ℞. and citrate of potash thrice daily; and somewhat experimentally at first, citrate of caffein gr. iij. in pill at night. The first night after taking the pill he had rest and repose, and the quantity of urine passed during the night and following morning amounted to two and a half pints. After the lapse of a few days, however, the quantity of urine began again to diminish, and the heart's action became more tumultuous and irregular; the digitalis mixture was therefore omitted, and the citrate of caffein in gr. iij. doses was ordered in solution with glycerine and water thrice daily. The urine had risen in quantity the next day to two pints, and during the six weeks in which the citrate of caffein mixture was regularly continued, the urine varied in quantity from two to three pints in the twenty-four hours. My notes also record on September 2, "Heart's action more regular, less tumultuous, and comparatively more powerful." On September 9, "Heart's action regular, patient relieved and more comfortable, with a better appetite, and the state of general anasarca much lessened and rapidly disappearing." It may be said that under the use of the citrate of caffein the extreme urgency of symptoms under which the patient laboured, and which rendered the prognosis a grave one from the outset, passed away, and there was established a state of repose, and a temporary state of power, which the ordinary form of treatment by digitalis and diuretics failed apparently to produce. Considering, indeed, the existence of mitral and aortic insufficiency with a large dilated heart, undergoing progressive mural decay and an embarrassed neurosal muscular action, little more could perhaps be expected. The case eventually succumbed to superadded attacks of angina pectoris, congestion of the lungs, and rapidly recurring general anasarca, and the autopsy established the correctness of the primary diagnosis.

Case 2.—Anne T., aged 47. Admitted into the Exeter Hospital, June 28, 1878. Bronchitis: brawny oedema (simulating elephantiasis) of legs with erysipelatous inflammation which had followed puncture, general anasarca, ascites, and considerable hepatic enlargement. Examination revealed mitral incom-

petency with a large dilated attenuated heart; a rapid, irregular adynamic, and inco-ordinate cardiac action, and a small rapid irregular pulse; the quantity of urine passed in the twenty-four hours amounted to one pint; it was acid, high coloured, but free from albumen. She is a stout woman with dusky complexion, and sits up in bed breathing hurriedly and in apparent distress.

The effect of digitalis combined with other diuretics in this case appeared to be to increase the quantity of urine passed to a slight extent, but otherwise an increase of the cardiac distress and an alarming retardation of the heart's action was induced—digitalis combined with stimulants produced no better results. A fortnight after admission, gr. iij. citrate of caffein was ordered in pill every night. On the first night the pill was taken she slept better, and towards morning passed what she herself described as a large quantity of urine, but owing to misunderstanding it was not measured. The next day the quantity passed in the twenty-four hours under the use of the caffein was four pints, and with these results before me the digitalis mixture was stopped and the caffein in pill ordered every four hours.

During the four weeks that the caffein was used the quantity of urine varied from three to four pints, and the practical results were, subsidence of all œdema and ascites, and even the legs assumed their normal size, the action of the heart became more forcible and regular, and the patient became so far convalescent as to be getting up daily. Latterly the caffein appeared to induce sickness, and it was therefore discontinued, and small doses of iron with digitalis were looked to to establish the work begun. There remained, however, a large dilated attenuated heart, showing every sign of muscular embarrassment, and an incompetent worthless mitral valve, together with a liver enlarged to below the umbilical line, and occasionally urgent dyspnoea, and some bronchitis. The case ultimately succumbed to a sudden attack of congestion of the lungs three months after admission, and during a time when progress appeared satisfactory. The autopsy verified the cardiac condition diagnosed.

Case 3.—John L., aged 63. A thin, spare, ill-nourished man, with a systolic mitral murmur, and a feeble irregularly acting heart. The apex of the heart was displaced to the left, the pulmonary second sound was accentuated, and a soft blowing murmur accompanied the first sound in the tricuspid area. Urine scanty and albuminous; lower limbs œdematous, pain and tenderness on pressure, and dulness on percussion in the epigastric region, for which he stated he had been sent into the hospital on the view that there was some disease of the liver.

The case appeared to be one of mitral insufficiency, congested right side, and congested portal system, together with a heart commencing to undergo mural decay.

On the third day after admission the following mixture was ordered:—

Rx. Caffein citratis gr. iij., glycer. 3 ss., T. pruni virg., 3 ss., aq. (ad) ʒj. ft. H. t. d. s.;

and a pill of podophyllin and aloes every other night.

The day following the quantity of urine had risen from one pint to two and a half pints, and the subsequent daily account gives, (August 28) three pints, (29) three pints, (30) two and a half pints, (31) two pints.

The urine was now clear and contained no albumen. The heart's action was more forcible and regular, hepatic dulness, tenderness and pain had diminished, appetite was returning, and he expressed himself as considerably better than he had been for months. During the time that the caffein mixture was continued the urine varied in quantity from two to three pints in the twenty-four hours, and iron and digitalis was finally looked to to establish convalescence. The case was discharged relieved.

Case 4.—Elizabeth R., aged 56. Stout, but healthy in appearance. Complains of weakness, giddiness, and enlargement of the bowels, with puffiness of legs. She states that she has only been growing so stout within the last six months, and at the same time she noticed that shortness of breath on exertion and momentary attacks of giddiness affected her. Examination revealed an aortic systolic murmur, with the first sound relatively feeble at apex. The amount of urine passed in the twenty-four hours amounted to one pint, contained no albumen, but was loaded with lithates.

A mixture containing gr. iij. of citrate of caffein in solution with glycerine and water was ordered thrice daily. For the next two days the quantity of urine passed was doubled, and it became clear; sickness then supervening, the mixture was discontinued for one day; the urine fell again to one pint, but rose again to two pints under the following day's use of the caffein. Sickness recurring the caffein was then discontinued, and the case offers no other point of interest.

The four cases enumerated will probably suffice to illustrate the value of citrate of caffein as a safe and efficient diuretic. Cases 1 and 2, it will be observed, were urgent, and they were, moreover, cases in which digitalis and diuretics appeared, for reasons readily understood, to be of no avail. Case 3 had no special urgency, but essentially the same pathological state was present; and Case 4, illustrates by itself the therapeutic position of the drug as a simple excitor of renal secretion; the

remaining points indicated are, the tendency of the drug to excite sickness in some cases, and the general sedative effects it appears sometimes to induce. My observations also lead me to the opinion that citrate of caffein occupies a "special" place as a therapeutic agent in cardiac disorder, and this is of course dependent upon a clinical hypothesis of the physiological agencies whereby its remedial effects appear to be induced. This "special" position of the drug as a cardiac diuretic appears limited to advanced cases of cardiac disorder, or such advancing cases of cardiac disorder where muscular embarrassment and neurosal inco-ordinate cardiac action (the indicators of progressive mural decay) appear clinically to forbid the administration of "tonic" doses of digitalis so long as an outlet has not been effected from the venous system which shall, coincidently with more powerful cardiac contraction, relieve the stagnating pressure of venous blood on the right side of the heart. It is to (1) diuretics establishing a drain on the general circulation, and indirectly staying the congestion of the renal veins in their connection with the inferior vena cava; and (2) to drastic purgatives acting on the valveless portal circulation, and so directly unloading the stagnating venous system; that we usually look for performing the necessary function of establishing equilibrium in the cardiac and general circulation. If we can effect this by either or both of these means the heart at once gains power irrespective of tonics to excite its muscular action, because its source of embarrassment is removed, and the way is, so to speak, paved for the action of "tonics" as safe and efficient remedies. Now, according to Binz (*Elements of Therapeutics*), "Caffein in moderate doses increases the heart's action, both by its direct effect on the organ, and also by exciting contraction of the arteries. The blood-pressure and the frequency of the pulse are intensified. More urea and carbonic acid are excreted and urine secreted than in the normal state, but all these symptoms are of relatively short duration." In other words citrate of caffein is (1) a diuretic or excitor of renal secretion, and the establishment of this effect is of primary importance; and (2) it increases the heart's action either (a) directly by stimulating the organ itself, or (b) indirectly by means of the arteries which it also excites to further contraction. Both these latter effects, by which the heart is controlled, are of great interest and importance; it is clinically probable that the first effect, that of direct stimulation of the heart itself, is produced through the agency of the sympathetic system, and specially of the intrinsic cardiac ganglia, for it is in the early, progressing, and later stages of mural decay that the drug appears to exert its influence; in the early, when the direct stimulant will enforce

greater contraction, and therefore stay the enfeebling compensatory power; in the later, when a state of adynamy and muscular embarrassment calls for an unloading of a stagnating circulation, and a stimulation of a muscle weakened from over-work. It is not always, indeed, that apparent increased cardiac action demands a sedative such as digitalis, aconite, or prunus virginiana: adynamic palpitation, which is really the result of inco-ordinate muscular action, or a want of associative action amongst the intrinsic cardiac sympathetic, will only be met by stimulants added to diuretics, that for the time promote blood-pressure and stimulate the renal organs to independent action. The intrinsic cardiac ganglia are physiologically of the utmost importance, for they endow the heart with an inherent power of its own, and enable us to account for the occurrence of palpitation when apparently other conditions are adverse to such increased movement: adynamy and arterial relaxation may exist, but yet there is palpitation of the heart; the pulse, moreover, is no criterion of the extent or state of its existence, and this because, in addition to the plexus of the great sympathetic with its connecting media to the spinal ganglia and cord, the heart exhibits in itself the excitory ganglia (specially of Remak and Bidder) stimulated by sensory centripetal fibres in the endocardium, and left uncontrolled by the inhibitory interauricular ganglion of Ludwig, which has been experimentally shown to be unable to counterbalance the motor power of its antagonists. Therapeutically, then, we want to relieve a stagnating circulation, and to stimulate the cardiac ganglia reflexly excited through pressure of blood on the endocardium to co-ordinate action. This double effect is induced by citrate of caffein.

Caffein, again, it has been said, excites the arterial vessels to contraction. Like belladonna it probably acts by overcoming the controlling effect of the splanchnic nerve, and through this medium, that of the abdominal vascular system generally, but whether this is induced directly on the vessels, or indirectly as a reflex act commencing in the sensory nerve (depressor nerve of Ludwig and Cyon) of the heart itself is practically not very material: suffice it to say that caffein similarly to belladonna appears to increase vascular pressure, and consequently promotes cardiac contraction through the medium of the splanchnic nerves.

Whatever may be the true theory of the action of citrate of caffein, the opinion is practically forced upon me through marked success in certain cases of the same type, and equally marked failure in other cases of a different type, that the caffein in doses of from gr. iij. to gr. vj., is a diuretic and cardiac stimulant of great value in such cases of cardiac dropsy

where a dilated, feeble, and irregularly contracting heart undergoing progressive mural decay is the main clinical and pathological element to be contended against. — *Practitioner*, January, 1879, p. 23.

41.—SOME REMARKS ON OBSTRUCTION OF THE BOWELS.
By Dr. T. CLIFFORD ALBUTT, M.A., Physician to the General
Infirmary, Leeds.

When a doctor is called to a case of obstruction of the bowels, he has at once to determine, if possible, the process, the site, and the nature of the block. As regards the process of it, he will try to decide whether the obstruction be due first to enteritis, plugging, intussusception, hernia, or stricture. He may then decide where the disturbance lies, and finally of what nature it is. The enteritis may be primary, secondary, or tubercular, &c. Plugging may be fæcal, polypoid, due to gall-stone, and so forth, and may be accompanied by enteritis. Intussusception may be strangulated, or simply incarcerated, and is probably always accompanied by inflammation, simple, purulent, or gangrenous. Hernia may be in the usual external or internal sites; or may be due to other accidental twisting or snaring of a loop of bowel, and is sure to be accompanied by some degree of inflammation. Finally, stricture may be simple, malignant, or due to pressure from without; and is less frequently accompanied by inflammation than the preceding events. With all the other forms of obstruction, inflammation of the bowel is more or less surely associated.

Now, we know that simple local enteritis may of itself cause the symptoms of obstruction, as, for instance, in cases in which a loop of open bowel is involved in an inflammation of parts beside it. So that, if we decide that we have to do with plugging, intussusception, or hernia of the bowels, we may assume that we have to do with some measure of enteritis over and above; and that this enteritis, acting by palsy of the intestine at the part, is to be added to the cause or causes of obstruction. We have to decide, then, whether the obstruction be due to inflammation alone, or to plugging, intussusception, or hernia (internal or external), accompanied by inflammation, and whether this latter be more or less, and in what stage, or whether the obstruction be due to stricture.

The questions of site and nature are not essential to the matters more closely before us. One other very important distinction, however, is closely before us, and was discussed at Bath by several speakers, and at Huddersfield especially by Mr. McGill, namely, whether the obstruction be acute or chronic. Under this head, I wish to draw attention especially to the fact that *early acuteness* depends much upon the nature and rate of

the cause; and to this other side of the same thing, that chronic obstruction often puts in an acute appearance. This I may call *late acuteness*. I have found in practice that there is a danger of mistaking late acuteness for early acuteness. I need not record cases, as they are not uncommon, in which malignant disease of the bowel or its neighbourhood is suddenly revealed by symptoms of acute obstruction occurring in a person hitherto seemingly in good health. In such a case, intussusception, volvulus, internal hernia, and the like, may be suspected when the real cause is malignant stricture. Such an error may, I believe, be generally avoided if the possibility of it be before the practitioner. In each case, we shall probably find some symptoms to aid us, and two especially I have found of use; firstly, that before such obstruction becomes acutely manifest, its earlier degrees have been counteracted by hypertrophy of the gut above the point, so that in moderately thin people coils of intestine may be seen and felt upon the abdominal surface, and may be brought into still greater prominence by manipulation; secondly, that, as a consequence of this hypertrophy, the injecta are swept too hastily down to the seat of obstruction and are forced through it in great measure unabsorbed. Such persons lose flesh in spite of fair appetite, and will, if questioned, speak decidedly of the great increase in the volume of their stools for some time previously. Violent ejections of wind from the rectum are also noticed in the history of such cases, and for the same reason. Malignant obstructions again in the lower colon may often be reached by a stem and ball, by the hand, or even by the finger in the bowel, and thus diagnosis is made positive. In the majority of cases, however, the nature of the stricture may be inferred with tolerable certainty from the previous history of the case, and from other present signs and symptoms. Obstruction from local inflammation is not so urgent nor so complete as in the other cases named.

Moreover, although peristalsis may cease at the spot, matters will drop by gravitation into the length below, and insoluble remnants of recently taken food may be recognised in the motions. Fæcal accumulation is generally to be made out by its history, symptoms, and signs. Gall-stone obstructions in my experience are always to be ascertained by reference to past hepatic colic; and polypoid obstructions resemble gradual stricture in their development, are fortunately rare, and are often within reach of touch. Intussusceptions and hernial obstructions alone remain, and intussusception is practically a hernia of bowel into bowel. Yet, happily, the distinction is generally to be made by reference to the age of the patient, and by searching the abdomen for a tumour, and the stools for blood. A past history of peritonitis would suggest the presence of bands, bridles, or dislocations.

In dealing with obstruction of the bowels, the lesson I am never tired of enforcing here, as in all cases, is that therapeutics depend upon diagnosis. Make diagnosis accurate and complete, and treatment will come. This is the reply students should receive time after time, when they will ask what is good for this disease or that disease. Never mind the name of the disease, but make out precisely what state of things you have before you in the individual case, and you will rarely be at a loss for treatment.

Obstruction of the bowels, then, may be really acute or really chronic; or again acute obstruction may occur in the course of chronic obstruction, and may even be the first revelation of it. With care, we can generally clear our case under this head. Chronic obstructions have to be dealt with according to the special features of each case, and we have plenty of time to weigh our opinions and to adjust our healing measures. When drugs fail us, the surgeon comes to our aid and makes an artificial anus. Acute obstruction occurring in the course of chronic obstruction is generally to be reduced to its chronic state by rest and subcutaneous morphia.

Let us, then, dispose thus hastily of all cases of essentially chronic obstruction. They rarely need heroic action or great presence of mind either in physician or in surgeon. We thus clear our way to the essentially acute cases; and we have found that in these inflammation always counts for something, generally for a good deal. Now, whether we know exactly how to deal with the main cause or not, we always know how to deal with the inflammation; and, except in the extremest urgency, our first duty is to simplify our case by lessening this. Unfortunately, means are often used which tend rather to aggravate the enteritis, and of all these injections into the bowel are the most mischievous. Even in the case of fæcal accumulation, it is not the fæcal mass, but the inflammation set up by it, to which the explosion is due, so that even in these cases it is wild practice to pump into the inflamed bowel and to drag the patient hither and thither in bed. Even for diagnostic purposes, enemata are rated far too highly, and are rarely of much service. Clearly, it is our duty to reduce our case by complete rest, opiates, fomentations, and a leech or two to the abdomen or anus until the enteritis subsides, and then we can deal as we see fit with its cause. But, if enemata are abused in fæcal accumulation wherein they are chiefly valuable, what are we to say of the fashion of forcing their employment in cases of internal strangulation? It is certainly conceivable that an intussusception may be unfolded by a forcible and ample injection, or by the insufflation of air, but to force air or water against a knotted or snared loop of intestine is surely outrageous meddling. And yet I

have never been called into a case of such obstruction without finding that such a measure has been assiduously employed, to the harass of the patient, to the aggravation of the symptoms, and to the increase of inflammation around the obstruction. I believe no formula can be drawn from our experience of more value than this, namely, that *if rest in every way be sedulously enforced, and the inflammation which palsies the bowel be carefully combated by the use of sedatives such as opium and belladonna, and other means, cases of obstruction of the bowels tend to recovery.* Experience, written and personal, is full of illustrative cases. At Bath, I ventured to quote the case which follows.

R. J., aged 13, fairly nourished, was said to have enjoyed good health. His family was said to be consumptive. On May 20th, 1878, he was suddenly seized with pain in the abdomen. This was increased by pressure; there was no vomiting. May 21st. Vomiting set in; the pain continued; temperature 103° . Those symptoms continued the same, with the addition of increasing tympanites until June 1st. On this day I met Dr. Wright, of Wakefield, in the case; Mr. Walker, the family attendant, being unfortunately absent. Dr. Wright and myself found pain, vomiting, tympanites, tenesmus, and an escape of mucus from the anus. Blood had come away at times, but not for a day or two. The anus was deeply congested, wet with mucous discharge, and gaping widely. A large intussusception could easily be felt in the rectum, and the finger passed round it. Dr. Wright and myself consulted anxiously what was to be done, and the use of taxis under ether was much discussed. As the case, however, was of ten days' standing and the pain and fever considerable, we feared the parts were at least greatly inflamed, and possibly gangrenous. Believing that rest and antiphlogistic measures were imperatively called for, we determined for a while to let all alone and to give opium as fully as the symptoms should need, and to apply leeches to the anus if required. Under fuller opiates, the acuter symptoms so much diminished that no further measures were taken, and we hoped the intussusception might follow some natural mode of recovery, as, for instance, by slough.

Mr. Walker wrote to me in September 1878 as follows: "Two days after your visit, the intussusception had passed out of reach; and a day or two later he had an attack of stercoraceous vomiting lasting a few hours. After this, he steadily and continuously improved and regained health and strength. No portion of intestine has ever come away."

Such is the very brief outline of this remarkable case, which helps to prove plainly that the first step to relief in intussusception must be to reduce inflammation and to diminish the forcing downwards of the gut. I have little doubt that forcible measures would have killed the lad.

Finally, as to the therapeutics of acute obstruction due to twists of the bowel, to snaring of loops of bowel in bands or fissures, to knots in its length, and to similar accidents; let us first clearly see that in reality, if not in name, all those events are hernias, and must be treated as such. Do we hustle the patient and his gut with injections in femoral or umbilical hernia? Surely not. Why, then, do we do so in internal hernias? Is it not our business to soothe with fomentations and the like; and, above all, to put the patient fully under opiates?

Meanwhile, taxis can be used at discretion. In such internal hernias, then, as we are speaking of, let us remember that our duty is to put the bowel at rest and the patient under opiates, knowing that in these cases also we shall promote the escape of the bowel more readily than by irritating it. To whip a dog in a leash is to strangle him; he may slip his collar if he be left to himself. It is not by agonised writhings that the bowel will get loose, but by quiet subsidence, if at all. When the patient is under full opiates, gentle taxis upon the walls of the abdomen should be used in all directions. But, if those means fail, the abdomen must be opened, as it must in strangulated hernia. As in the latter case, operation gives the patient his only chance.

Here we reach the issue between Mr. Hutchinson's canons and the teaching of the Leeds surgeons; and I cannot hesitate to support the practice of my colleagues in this matter. I repeat, let us make our diagnosis certain; and if we can do this, and for the most part we can by close attention to the history of the case, the signs, the symptoms, and the age of the patient, then I say the indications for operation will be as clear as in the case of strangulated external hernia. To deny operation to a case of internal hernia is, in my opinion, to deny the patient some fair chance of life; and for confirmation of this view, I rely upon Mr. Teale's paper, which, I believe, represents the views also of Mr. Wheelhouse, Mr. Jessop, and Mr. Atkinson; the two latter of whom spoke at Huddersfield, and will, I hope, publish their cases and opinions.—*British Medical Journal*, Jan. 11, 1879, p. 40.

42.—ON EXPLORATION OF THE ABDOMEN IN OBSTRUCTION OF THE BOWELS.

By T. P. TEALE, Esq., M.A., Surgeon to the General Infirmary, Leeds.

The recent debate on this subject at the meeting of the British Medical Association in Bath, and the expression of an opinion unfavourable to exploration of the abdomen in obstruction of the bowels by one of our most thoughtful and observant

metropolitan surgeons (Mr. Jonathan Hutchinson), shows that the question is still far from being settled, and imposes an obligation on those who have had experience in the subject to contribute what evidence they possess towards the solution of such a serious and difficult question.

There can be no doubt that the subject has been looked upon with much favour by the Leeds surgeons; and I must confess to having myself a strong leaning towards the operation, on the grounds both of theory and experience.

As far as I can make out from my notes, or recall from memory, I have six times opened the abdomen in apparently hopeless cases of obstruction of the bowel; and I do not consider that in any one of them a chance of recovery was taken away by the operation. In one of the six cases, I believe life would have been preserved if I had operated two days earlier.

Two cases, at least, I have seen in which section of the abdomen would have revealed the true nature of the disease and would have given a reasonable chance of preservation of life. In the first, a patient died of obstructed bowels, who had previously recovered from an operation for strangulated hernia. The cause of death was strangulation of the bowel by a band uniting two coils of intestine. This, of course, could easily have been set free. In the second, the patient died from peritonitis caused by rupture of the bladder. Being convinced that the bladder was ruptured, I proposed to explore; but, as my colleagues did not think the symptoms sufficiently pronounced to justify what we then looked upon as a most hazardous operation, I desisted. The man died in about two days, and a rupture in the fundus of the bladder was found. Had an operation been agreed upon, it was my intention, first, to stitch up the rent in the bladder; second, to wash out the peritoneal cavity; third, to perform perineal section, so as to secure a more ready and perfect drainage by a large tube than could be obtained by a catheter in the urethra.

Exploration of the abdominal cavity is, to my mind, justifiable on two cardinal grounds. Firstly, the mere opening of the peritoneal cavity is not necessarily a dangerous operation; secondly, there are many cases of obstruction of the bowels which must prove fatal unless relief can be given, which can only be rightly directed by means of exploration of the abdominal cavity.

As to the impunity with which the peritoneal cavity may be opened, I need hardly remind you how constantly this is proved in operations for hernia. We think nothing of pulling out and handling coils of intestine; and we rarely look for danger unless the bowel have been damaged by too long continued strangulation. It is a common saying, that it is not the opera-

tion which proves fatal, but the delay in operating; in other words, the damage to the bowel by delay, not the damage to the peritoneum by the operation.

Again in ovariectomy—What are the dangers there? Mainly from the fluids poured out from adhesions and divided vessels, dangers which, as a rule, are absent in gastrotomy. Look at Keith's thirty-nine cases of ovariectomy without a death. Look at the confidence with which he reckons upon recovery where there is a smooth peritoneum without adhesions. Why need we have such dread of opening the peritoneal cavity?—*British Medical Journal*, January 11, 1879, p. 41.

SURGERY.

AMPUTATIONS, FRACTURES, DISLOCATIONS, AND DISEASES
OF THE BONES, JOINTS, ETC.

43.—ON CASES OF SPINAL DISEASE TREATED BY SAYRE'S JACKET.

By BERKELEY HILL, Esq., M.B., Surgeon to University College Hospital, and to the Lock Hospital, London.

[The following cases, which were related to the London Clinical Society, comprise all that has been written on Dr. Sayre's plan of treating spinal diseases. Mr. Hill says :]

In bringing these cases forward, it is not my intention to enter into a discussion of the correctness of the theories which Dr. Sayre advanced to explain the manner in which this immovable case effected the results he claimed for it; nor to describe the method of application of the jacket to cases of angular and lateral curvature, which is probably more or less familiar to all present. This is fully set forth by Dr. Sayre himself in his work on Spinal Diseases and Curvatures, 1877. I desire to speak rather of the results arrived at in six months' experience of his method. For this reason I have collected several patients wearing the jacket—some of them only recently encased, it is true, others who have worn the shell a longer time. Some of them have been under my own care, some of them under that of my colleagues, Messrs. John Marshall, Marcus Beck, and Arthur Barker. Four most interesting cases are contributed by my friend Mr. Jas. P. Bartlett, Surgeon to the Cheney Hospital, Cheney Walk, Chelsea.

The earliest case treated after Dr. Sayre's demonstrations at University College Hospital, on July 9 and 12, 1877, is here to-night. She is a young woman of 23, and has worn her support since last July 13. In 1876 she had been under my care at University College Hospital for psoas abscess, and angular curvature of the lower dorsal region. The psoas abscess was drained and healed, and the patient left the Hospital with directions to lie down for six months. This direction was carried out with more or less exactness, and a year thus passed away. Just at the time of Dr. Sayre's arrival in this country she applied again at the Hospital, complaining that the old pain in

the back," of which she had been quite free for months, had returned, and was then so severe as to prevent her from moving about or even sitting up for more than a very short time. On examination, pressure on the free ends of the floating ribs and in the vertebral groove of the right side caused considerable pain. There was no fulness in the lumbar or iliac regions, or evidence of a fresh accumulation of abscess. There was prominence without tenderness of the 11th and 12th dorsal and 1st lumbar spines. When suspended in the gallows she was relieved of her backache at once. On July 13, the house surgeon, Mr. Frank J. Davies, and the dressers applied the jacket in my presence. As soon as the plaster was set the patient was allowed to go to her own home close by, with directions to remain quietly in bed till the next day, when we would alter any tight or chafing parts, should they exist. She came again the next day, complaining that the jacket, which had been carried too high, cut her under the arms, and that it was generally but not unbearably tight. Notwithstanding these annoyances she had felt so strong and comfortable in her back that she had, instead of going to bed when she left us, taken a walk down Tottenham Court Road to see the shops, a pleasure then long denied to her. The edges of the jacket were pared away and covered with tinfoil, and the patient then went into the country. In November she returned to London much better, accustomed to sit up all day long, and latterly much engaged in nursing a sick sister. For more than three months after the shell was applied she had been free from backache, but latterly, as she had been at harder work, her back had begun to be a little painful if she did not lie down and rest it now and then. The plaster case, which was as firm and as good a fit as when applied in July, was cut down a little before Christmas under the expectation that a lighter and movable case would give her sufficient support. Pressure in the vertebral groove and on the floating ribs caused no pain at all. To this end Mr. Coxeter has adapted a poro-plastic felt jacket, moulded on to the patient while she was suspended in the gallows. Either this does not give her sufficient support, or she presumes too much upon it, being occupied all day long in house-work or in sewing, for she is not now free from backache, except before rising in the morning, when she is perfectly comfortable. Probably a fresh application of the plaster jacket will be necessary. Nevertheless the mode of treatment may be said to have answered all reasonable expectations. The patient has progressed steadily towards cure, while able to lead a moderately active life and greatly improving in her general health.

The notes of the second case put up in University College Hospital, I am permitted to read by Mr. Marcus Beck, who was

the operator on this occasion :—"K. M., æt. 16, a delicate boy, growing rapidly; very tall for his age; was at school in Switzerland. In March 1877 was seized somewhat suddenly with pain in the back. Pain increased rapidly. Was said to be lumbago. Any movement caused 'agonising cramps in the stomach.' Had been in bed all the time. In six weeks from the commencement of the attack he was able to be moved to Aix-les-Bains, where the disease was for the first time diagnosed as '*Maladie de Pott*.' He took baths and was able to get about on crutches. On July 14, 1877, that is, four months after his first seizure, he had distinct angular projection of the spines of the last dorsal and first lumbar vertebræ. Could walk on crutches, but soon began to feel pain in the back. Could not walk 50 yards without crutches because of the pain in the back. He was put up in Sayre's apparatus, and at once abandoned his crutches, carrying them away with him both in one hand and walking comfortably. July 16.—Went to the seaside. August 17.—Was walking about all day, complaining of nothing. October 19.—The same shell was still on, but quite soft and useless; could walk 9 miles a day; had gained flesh. Shell removed. January 22.—He has remained quite well ever since." (This case has been already published by Dr. Sayre in his *Spinal Diseases*, &c., p. 87, Case No. 30; but as the note there dated is not continued later than August 17 I have ventured to repeat it here with its conclusion.)

Dr. Gooding, of Cheltenham, has permitted me to allude to the case of his little girl, who was encased by Dr. Sayre before a large audience in Guy's Hospital, on July 25, 1877. This was a case of extreme severe and long-standing angular curvature of the middle dorsal region, with history of previous abscess and paraplegia (Case 28 in Dr. Sayre's book). In answer to my inquiry Dr. Gooding has very obligingly written to me an account of his child's progress up to January 23, 1878:—"The health began to improve from the first, and she was able to move about without pain, and has continued to walk with increasing strength ever since. She has literally had no pain at all. She does her share of play, joining and enjoying children's parties, and going regularly to school."

This case was almost hopeless—getting steadily worse, more and more deformed, and in constant misery for eight years, until she was encased in the plaster-jacket. Immediately this was done she began to mend, and has made steady advance ever since.

I will now enumerate the advantages which six months' experience of Sayre's treatment has established.

1. Pain is arrested immediately.

2. So far as his spine is concerned the patient is at once able to sit upright and to walk about.

3. Control of the lower extremities, when diminished by debility or paralysis, is rapidly restored in many cases.

4. Abscesses close.

5. The spinal column loses much of its abnormal curve, and consolidates in the improved position.

6. In lateral curvatures a permanent increase of stature is often obtained.

7. Finally, cure is more rapid, and ensues in a manner far less irksome to the patient than by any other method yet devised.

Nevertheless, it must not be supposed, as patients are too apt to do, that the plaster shell will enable the patient at once to behave as if he were already cured, and to indulge in any exertion or exercise that may please him. Indeed, Sayre's treatment has this disadvantage: it gives the patient such complete support that he rejoices in his newly-gained power of locomotion beyond the limits of prudence. Such cases as that of Mr. Beck's and some others which have come under my notice are exceptions. Though able to take sufficient exercise most patients require a good deal of rest and to be checked from too boisterous movement for some months after the jacket is applied.

One word regarding the employment of self-suspension in cases of long-standing and severe lateral curvature. I have endeavoured to inculcate on the patient and her friends the necessity for regular self-suspension in the manner laid down by Dr. Sayre, and certainly in many cases these directions have been carefully carried out. But I have not satisfied myself that the back has obviously straightened during the time they have been under observation. The jacket has propped up the back and preserved the straightening immediately gained by the first suspension, but further progress from this first advance has not been observed by me. It is to be understood that I limit these remarks to cases of very advanced lateral curvature. Early cases, which are rare among hospital patients, dislike to submit to what appears to them more irksome and painful than the malady it is intended to cure. Such persons it is difficult to persuade to be wise in time.

During these repeated applications of the plaster-jackets several small modifications have been tried, and some found to be beneficial.

1. The chin-piece is a frequent trouble. We have found it useful to cut the part which receives the chin more deeply than were the original patterns. Also to line the leather with thick soft felt. The suspending straps should be attached just in front of the zygoma, and just behind the ear, and hitched on to the ends of the stretcher, not to the notches half way along.

Thus pressure on the chin, jaws, and nucha is reduced to a moderate amount.

2. The best axillary slings have a hard firm pad, resembling the cross of a crutch, such bear least disagreeably on the vessels and nerves.

3. The cost of the self-suspending apparatus is materially lessened by replacing the tripod-stand with a pulley screwed into the ceiling, through which a rope is rove; the chin-strap and stretcher are all that are then required.

4. In adjusting the skin-fitting vest it is well to lace its upper edges together with a tape carried backwards and forwards over the shoulders before the lower border is fastened below the hips and perinæum.

5. Fitting the pads requires great care—every bony prominence must be protected by making a soft one of cotton wool project beyond it on either side, not *over it*. In lateral curvature the hips, especially that of the side opposite to the curve, are apt to gall. This is best prevented by a Welsh flannel roller carried round them several times outside the vest, or by a layer of thick felt. Cotton wool is apt to get lumpy in time and lose its effect.

6. It is most important to carry the lower edge of the shell nearly as low down as the pubes in front and the sacrum behind, just escaping the great trochanters, arching it a little at the groins lest it incommode the patient when he sits upright.

7. With adults or heavy patients it is difficult to lay them down on the air bed while the plaster is setting without bending or breaking the shell. To do this easily and without risk Mr. Mayer has made for me a table to which the air bed is attached. This table swings at the middle on two uprights, like an ordinary toilet looking-glass. It can be quickly applied in a vertical position to the patient's back while he hangs. Two broad hooks are slipped under the arm-pits and then hitched to the top of the table. The gallows is set free, and the table, carrying with it the patient, is swung over from the vertical to the horizontal position, when he sinks into, and is securely supported on both sides by, the slack air bed. Mr. Mayer has also made a frame, to be used in the same manner, which can be taken to pieces and folded into a small parcel.

8. Cutting up the shells, or trimming them, is a troublesome business; a small stiff-backed carpenter's saw cuts very cleanly; and Mr. Phillips, house surgeon at University College Hospital, has introduced the French vine-dresser's shears, which is the best we have yet tried. They may be obtained from Mayer and Meltzer.—*Clinical Society's Transactions*, vol. xi., 1878, p. 80.

44.—TREATMENT OF ANGULAR CURVATURE OF SPINE BY A PLASTER-OF-PARIS JACKET APPLIED IN THE RECUMBENT POSTURE.

By Dr. THOMAS JAMES WALKER, Surgeon to the Peterborough Infirmary.

Thirteen years since, in the summer of 1865, I read before the Cambridge and Huntingdon Branch of this Association notes of a series of cases of Pott's Disease of the Spine treated by a gutta-percha jacket, applied in the recumbent posture, according to a method which I then described. This jacket I continued to employ up to the time when Dr. Sayre published the account of his treatment of curvature of the spine by a plaster-of-Paris jacket, applied while the patient was suspended by the head and shoulders. Until he demonstrated that the rigid jacket might with safety completely encase the body, I believed, as did Dr. Sayre up to 1873, that it must be divided down the front to admit of respiration; but, the material with which the gutta-percha jackets were laced being a stout inelastic stay-lace and not an elastic material such as Sayre used in his open jacket, my apparatus formed a more complete casing than his. In the paper to which I have referred, I said: "The jacket, when properly moulded by the many-tailed bandage and laced tightly up the front, must resemble two truncated cones, joined at the waist by their apices, the lower shallow one resting over the hips, the upper deeper one inverted, accurately adapted to the thorax and supporting the upper part of the body altogether, independently of the spinal column." Although such a jacket forms a very perfect support, retaining the spine in a proper position, and being readily removed and reapplied, it has the disadvantage of being made of a material requiring much nicety in its management and liable to be spoiled, if the surgeon be not practised in its manipulation. It is therefore only in exceptional cases that it should be preferred to the plaster-of-Paris jackets, the application of which I am about to demonstrate before you.

Of the great success which attends the treatment of caries of the spine by fixing and supporting the trunk in a proper position by a casing of plaster-of-Paris, gutta-percha, or some similar plastic material, my experience during the last seventeen years is most convincing, and fully confirms the opinions published by those who have of late adopted such plans; but, in order that such a jacket shall be effectual, it is a *sine quâ non* that it shall be applied when the spine is in such a position that the diseased vertebræ shall be free from all pressure, and the deformity, if any deformity exist, be reduced to a minimum.

This condition is found, it is true, when the patient is suspended by the head and shoulders, as recommended by Sayre,

but it also obtains in an equal degree when the patient is laid flat on his back; and the same result as to increase of height, diminution of the deformity of the spine, and relief to pain and functional disturbance follows the application of the bandage, when the patient is in the recumbent posture as when he is suspended. This statement will surprise those who, experiencing brilliant results from Sayre's treatment, regard the suspension which forms a part of it as its essential element, instead of viewing it, as I do, as a hazardous complication of the method. I must direct your attention for a few minutes to the changes occurring in the spine in this disease, as confirming the view I propose.

The spine is a jointed column in which each vertebra in health rests evenly on that below it. The first effect of inflammation of a vertebra leading to softening of the bone is that, in consequence of the superincumbent weight, the diseased bone becomes attenuated at its softest anterior portion; and, as it were, a wedge-shaped piece, broad in front and tapering off behind, being removed from the jointed column, the portion above must fall forward; but, being *jointed*, it not only leans forward, as would a rigid column, but the relations of the vertebræ to each other being disturbed and the intervertebral substance admitting a certain amount of bending at each joint, so soon as the vertebræ cease to rest firmly one upon the other, the whole of that part of the spine above the diseased bones is bowed forward. In order to compensate for this curving forward of the upper part of the spine, and to preserve the balance of the body, the spine below the diseased bones is directed backwards, and the deformity now consists not only of the sharp angle formed by the approximation of the bodies of the vertebræ above and below the attenuated diseased bones, but of the additional bowing forward of the parts of the spine above; the latter curve being resisted, and yet another compensatory curve being established in the opposite direction, by the action of the erector muscles of the spine. As soon as this deformity commences in the slightest degree, the bodies of the diseased vertebræ are exposed not only to the direct downward pressure of the upper part of the body, but are squeezed in the angle of the curved spine, as is a nut in a pair of nut-crackers, by the weight of the body, which, acting through the long arm of the lever formed by the upper part of the spine, acquires the mechanical power of a lever of the second order, the fulcrum being at the articular processes of the diseased bones.

It is this additional pressure which causes the rapid, almost sudden, development of the angular projection, in cases in which the inflammation of the body of the vertebræ may have existed for some time, without the wasting of the anterior part

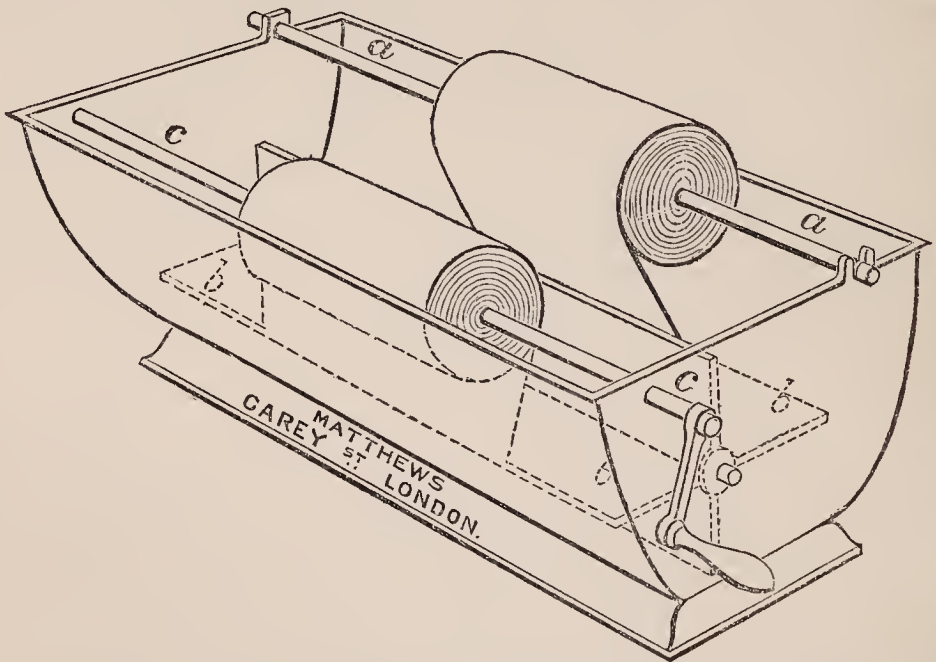
of the body of the bone which converts the direct downward pressure into the leverage described. A patient with such an angular curvature, if laid flat on his back, is speedily relieved from pain and distress. The spine above and below the diseased vertebræ being straightened out, the deformity is much relieved, the angular projection is less marked, and the patient's height is increased; at the same time, the carious bones are not only relieved from the downward pressure to which in the upright posture they were exposed, but are also liberated from the grip of the lever, while, the body being in a position of perfect rest, all abnormal muscular action ceases. This is the condition of the body most favourable to the cure of the disease, and this is the condition which we should maintain by the application of the plaster-of-Paris shell.

The same straightening of the spine by the opening up the curves above and below the diseased vertebræ may be obtained by suspension strictly carried out as enjoined by Sayre, only up to the point at which it gives relief and causes no pain to the patient; but, independently of other inconveniences and dangers, the favourable conditions described above as existing when the patient lies at rest cannot be obtained, even in an imperfect degree, by suspension, without some risk of disturbing and separating the diseased vertebræ. It is this risk which makes suspension in my opinion a dangerous as well as a complicated and inconvenient method of applying the jacket; and I will now proceed to demonstrate the manner in which I apply it in the recumbent posture; a proceeding for which I claim these advantages. The diseased bones are, at least, as perfectly relieved from pressure, the muscles are as completely relaxed, and the deformity is as much diminished when the patient lies flat on a bed as when he is suspended. These conditions are obtained without risk of injury, without terror, distress, danger of syncope, or any inconvenience to the patient; and a perfect jacket fixing the spine in the proper position for cure can be applied by the surgeon in his own consulting-room or in the patient's house, be it ever so small a cottage, without the help of any skilled assistant, and without a splash of plaster on his clothes, or even on the floor.

As I have elsewhere described, I formerly moulded the gutta-percha jackets by using a modification of the many-tailed bandage, and it is only on the same principle that a plaster-of-Paris jacket can be applied with the patient in a recumbent posture.

The best lining for the jacket is this closely fitting under-shirt recommended by Sayre. I have used occasionally a flannel bandage applied round the patient, or a sheet of cotton-wadding tacked like a shirt round the trunk, both of them being very imperfect substitutes for the vest. The bandage should be of

muslin; those I generally use are torn from a piece of Victoria lawn, nine yards long; the width must vary from two to four inches, according to the size of the patient. Plaster-of-Paris mixed with water alone, sets too quickly to admit of the necessary proceedings, and we must, therefore, add some material to retard the process of setting; the best, I believe, is the ordinary gum, and the materials must be used in the exact proportions of one pound of freshly baked plaster, one ounce of mucilage of gum acacia (*B. P.*), and eight ounces of water; with plaster mixed in these proportions, there is sufficient time to go through the details necessary to apply the jacket, and it will set firmly in from ten to fifteen minutes after it is applied. The most efficient, quickest, and cleanest mode of saturating the bandage with plaster, and that which I recommend you to adopt, is to use the simple machine which I have devised for



this purpose, and which is represented here: *a* is a spindle carrying the dry bandage, the end of which is passed under the fan roller *b b* to the spindle *c*. For a jacket for a full-grown boy, five pounds of plaster are gradually shaken into two pints of water, to which five ounces of mucilage have been added, the materials being well mixed; the bandage is now wound from the spindle *a a* on the to spindle *c c*, passing under the fan *b b*, causing it to revolve and stir the plaster, splashing it over both surfaces of the bandage; the box is wide enough to allow two full width bandages to be charged at the same time. It is necessary that all parts of the machine should work very easily that the bandage may be loosely rolled, or the plaster will be pressed out. This is the best method; but, not having my machine with me, I now employ the bandages rolled with dry plaster as recommended by Sayre, placing them in the water

and mucilage until they are sufficiently soaked. (Slips of bandage previously torn to the proper length may be steeped in the plaster mixed as directed above, then taken out, and, with the help of an assistant, smoothed and laid in position on the bed.) The bandage thus charged with wet plaster has now to be cut into slips of the length necessary to wrap round the patient's back, meet in front, and fold over for a few inches, and these slips have to be placed in proper position on the bed and in suitable layers for folding round the trunk of the patient so as to form a jacket reaching from below the crest of the ilium to the axilla. I, therefore, measure round the patient's chest, and take the depth of the jacket from the axilla to half an inch below the anterior spine of the ilium, and mark these dimensions on the bed, which I have protected by laying a few sheets of paper upon it. My assistant taking the end of the bandage, I rapidly unroll it across the bed, and with scissors divide it at the appropriate length, leaving the slip lying across the bed; the nurse again taking the end, places it so that the bandage as again unrolled shall overlap two-thirds of the slip previously laid down; I again cut it off at proper length, and we repeat the process until a layer of slips of bandage, each overlapping the other two-thirds of its width, is laid across the bed, of sufficient size to reach from the hip to the axilla. This will only give a jacket of the thickness of three layers of muslin, which is not sufficient; I therefore begin again at the bottom with the fresh bandages, first placing a narrow slip of paper across the bottom layer at each side, so as to prevent my confusing the ends of the bandages in the two layers during the next stage of the application. If I wanted a specially strong jacket for an active adult, I could repeat the process again, so as to form a third layer; but usually, if the bandage be well saturated with plaster, a thickness of six folds is sufficient, and, as each slip of bandage overlaps two-thirds of the one below it, this is obtained with two such sets of overlapping bandage.

The patient, who is stripped and clothed in the vest which is to form the lining of the jacket, will now lie down on the bed. I place him carefully, so that the edge of the jacket may come well below the crest of the ilium and not rest upon the bone; he raises his arms and lays them in such a position that the elbows shall just clear the top of the jacket (in the case of a female, the breasts must be held up, and pads of cotton wool placed so as to mould the plaster in a proper form to receive the breasts when the pads are removed), and lies down on the strips of bandage; I now take one end of the last slip laid down, while my assistant takes the other, and bringing them smoothly round the side we cross them tightly over the chest; we repeat this with each slip until we come to the bottom of

the first layer ; then, lifting the slips of paper placed to distinguish the two layers, we commence again with the bottom stratum, smoothing the whole over with what wet plaster remains. I have never found it necessary to use what Dr. Sayre calls the dinner-pad. The patient may now put his arms down, that I may, if I find it necessary before the plaster has set, cut away or fold over the edge under the arms ; in the same way, I fold up the ends of the lower slips of the bandage, and cut away the lower edge of the jacket if I find that it is so low that it will catch the thigh of the patient when he sits down. From the folding over of the slips in front, the jacket is at this part twice as thick as at any other ; it has a strong wide rib down the front, which is the point at which it should be strongest to resist the tendency of the spine to curve forward ; should it be thought desirable, it is easy to strengthen the back by placing a few strips of well-charged bandage vertically down the middle of the jacket before the patient lies down upon it, or by pouring in a little wet plaster between the layers of bandage.

You will observe that the addition of the mucilage has given me ample time to complete the jacket, but all must be done with a certain degree of rapidity, or the plaster on the first slips laid down, which are, of course, the last to be folded round, will be found to have set. In a few minutes, the lad will be able to get up, the jacket being completely hardened, and you will see that the spine is fixed in its straightened condition as completely as it would have been if the boy had been suspended by his head instead of resting easily on his back.

I do not propose to detain you by entering into any account of the cases to which the method I have demonstrated is applicable ; but I may state that I believe it to be adapted to every case in which Sayre recommends suspension ; the jury-mast can be fixed as well in this way as by suspension ; and the direction in which the slips of bandage are laid and folded may be modified according to the situation of the disease. Even in lateral curvature, the spine is much straightened, and the twisting remedied when the patient is laid either supine or prone ; and what is gained in straightness, and consequently in height, may be retained by such an application of the plaster-shell as I have shown.

Although my demonstration is intended only to illustrate the application of the apparatus in spinal cases, the practical surgeon will see at once that the method of applying by a many-tailed bandage plaster-of-Paris, the setting of which has been delayed by the addition of mucilage, may be available in many other cases where a solid immovable retentive apparatus is required. In conclusion, I would thus summarise the points which I wish to impress upon you by my demonstra-

tion, and the remarks with which I have accompanied it.

1. The main object of the treatment of angular curvature of the spine should be the maintenance of the affected bones and joints in a state of absolute rest, and that in the position most favourable for the cure of the disease without deformity.

2. This position is found when the patient is placed comfortably in a recumbent posture.

3. By the application of a plaster-of-Paris jacket, as recommended by Sayre, the bones may be fixed in this position, so as to retain it when the patient rises and moves about.

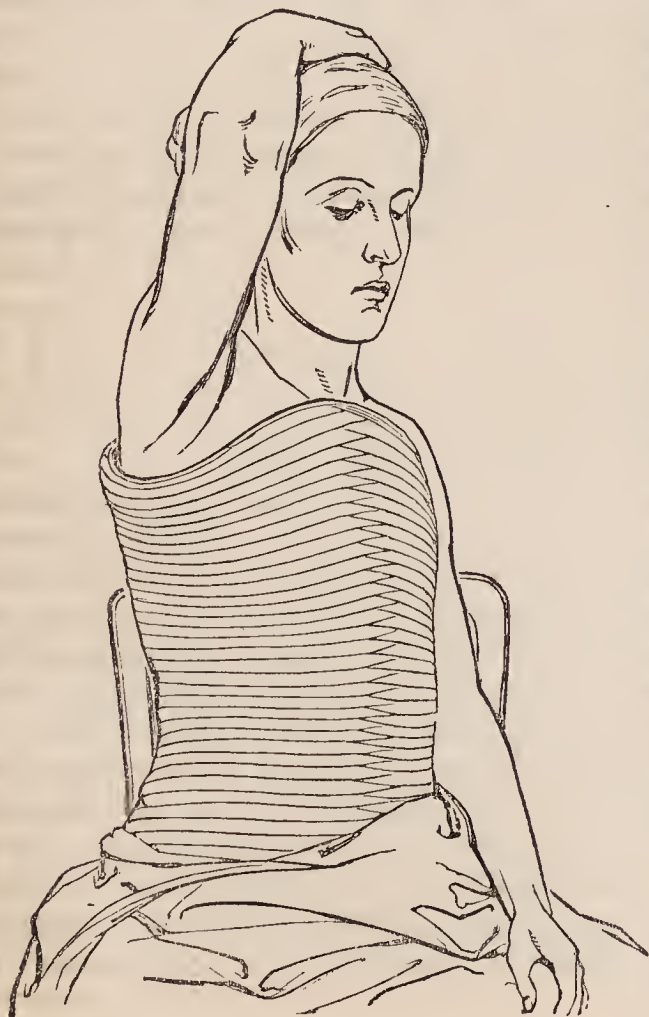
4. The only way in which such a jacket can be applied with the patient recumbent, is by the method which I have demonstrated.

5. This method depends for its practical facility on the application of the many-tailed bandage and the use of plaster-of-Paris mixed as I have directed.

6. The adoption of the recumbent posture dispenses with the inconvenience and serious risks of suspension, while all the advantages of Sayre's method are secured for the patient at a minimum of trouble to the surgeon.—*British Medical Journal*, March 1, 1879, p. 305.

This figure is from a photograph of S. W., aged 20, the

subject of angular curvature, the fifth dorsal being the most prominent vertebra. The jacket was applied as I have described, and the ends of the slips crossing over and being brought as high as the top of the sternum, the jacket is of double thickness down the front, thus giving a strong anterior splint, reaching the whole length of the trunk as high as the top of the sternum, and making it absolutely impossible for further bowing of the dorsal spine. No pads were used over either abdomen or breasts in the application of this shell, nor do I ever find them necessary; and the patient, who has worn the casing for two months, tells me



that she is comfortable, and that she is not conscious of wearing any apparatus except from her inability to bend her back in stooping.

The illustration shows the special applicability of my method of making the rigid shell to cases in which the curvature is situated high up in the spine. Where the disease is seated in the cervical vertebræ the jacket may be applied in the same manner, two iron eyes being fixed in front in the middle line, into which is slipped the leg of a Y-shaped splint, the upper arms of which, directed up on either side of the neck, are perforated at their extremities so as to allow an elastic band to be stretched across. On this the chin rests so as to carry the weight of the head, taking the place of the hands of the patient, which are so constantly used to support the chin, and prevent the head from falling forward. Or, if preferred, the jurymast apparatus recommended by Sayre may be adopted as readily as when the patient is suspended during the application of the bandage. I believe that if I was somewhat singular when, eighteen months since, I advocated and practised the use of the rigid casing, without suspension, I shall now be supported by the majority of English surgeons in the opinion that suspension of the patient is a part of Sayre's method which it is desirable to avoid, and that the application of the jacket in the manner demonstrated by me in August last at Bath, and on other occasions, is infinitely preferable, securing as it does, without risk to the patient or inconvenience to the surgeon, the fixing of the diseased bones in the most favourable position for obtaining a cure with a minimum of deformity.—*Lancet*, December 28, 1878, p. 915.

45.—ON EXCISION OF THE ELBOW-JOINT.

By THOMAS ANNANDALE, Esq., F.R.S.E., Professor of Clinical Surgery in the University of Edinburgh.

I would ask you to note a few of the general distinctions between excision of the knee-joint and excision of the elbow-joint. The first difference I would mention is in regard to the age of the patient. In the case of the knee-joint, excision is not advisable, unless in some very special circumstances, after the age of forty. In the case of the elbow-joint, the operation may be performed at any age. I have performed it successfully at the age of three years, and I have performed it also successfully at the age of seventy-five. The second difference I would ask you to note is that in the case of the knee-joint you require carefully to consider the question of interference with the epiphyses, especially the epiphysis at the lower end of the femur, because if it is involved in the disease, or requires to be

interfered with during the operation, the result is not satisfactory as regards the after-growth of the bone. But in the elbow the interference with the epiphysis does not appear to be of so much consequence. It is quite true that in a young person the arm may not grow to the same extent as the other arm, but still it may be a sufficiently useful limb. This, of course, refers only to young patients, before the epiphysis has become connected with the shaft of the bone. In the adult it is not of so much consequence. The third difference is, that in the case of the knee-joint, if there is any question between excision or amputation, you give amputation the preference; whereas, in the case of the elbow, amputation is rarely resorted to, and not unless in very aggravated circumstances, when the disease is very extensive or the condition of the patient is very bad. The last difference to which I would refer is that, in the case of the knee-joint, the result desired is a stiff and strong limb, which will take its part in supporting the body during progression. In the case of the elbow-joint, on the other hand, we desire to have a new and movable joint, one which will allow the movements of the limb to take place in as natural a manner as possible. So that, in the case of the knee-joint, the rule is to take away as little bone as possible, while, in the case of the elbow-joint, you must take away sufficient bone to give the limb mobility.

The next point in connection with this subject I would ask you to note is the different classes of cases in which excision of the elbow may be required.

The first class of cases are those of recent injury—compound fractures involving the elbow-joint, and compound dislocations, and in some cases, severe wounds opening into the joint. Now, all cases of this kind are cases suitable for excision, provided that the principal vessels and nerves are not injured, and also provided that the soft textures round the joint are not too much destroyed or lacerated; and, lastly, that the injury does not involve any large amount of the bone or bones. In connection with these cases of injury I would remark that in many instances the operation required is not a complete excision, because it generally happens that the injury involves principally, sometimes entirely, the one bone—that is to say, that in these cases of compound fracture or compound dislocation, it may be the lower end of the humerus only which is injured, or it may be the ulna, or the olecranon process of the ulna, or it may be the radius alone. It is not often that you meet with a case in which the whole of the bones of the elbow-joint are injured, and which admits of excision. In those cases the injury is usually so serious that amputation is required. It is therefore really the case that in the large majority of these

cases requiring excision, all you have to do is to remove the injured part of the bone. If the case is one of compound comminuted fracture, or compound dislocation of the end of the humerus, all you require to do is to remove the injured portion of the humerus. An important question arises here. Suppose that the injury is confined to one of the bones, and you remove the injured or protruding portion, should you perform a complete excision? My answer to that question is that you should be guided by the amount of bone which it is necessary to remove. If you find that you require to remove several inches of the bone, and if the other bones are not injured, then it would be sufficient to remove this alone, and to leave the other bones untouched. I have frequently removed in this way portions of one or two of the bones of the elbow-joint, and with very good results. In any case sufficient bone must be removed according to the principle presently to be referred to.

The next class of cases is disease of the joint. I should say that all cases are suitable for excision in which there has been prolonged suppuration of the joint, with disease of the articular surfaces or the articular ends of the bones. I would remind you that in these cases the disease sometimes affects one bone only, and not all of the bones, or it affects one bone more than the others. In addition to these cases, I believe that cases of chronic synovial disease, with prolonged suppuration, are best treated by excision when they have resisted all other treatment, and more especially if the disease is causing constitutional irritation. In removing the elbow-joint for disease it is well to take into consideration the following two points: first, that you have to take away the disease which is causing local and constitutional irritation: and, in the second place, that in doing so you have to provide the patient with an arm that will be movable and useful afterwards. Now I do not mean to say that you should perform excision of the elbow-joint in all these cases of disease, but I do say that the performance of the operation will give you, on the whole, the best results. The principal reason, which would make you urge an operation in any of these cases is, the effect which the disease is producing on the general health. In any case where it is producing constitutional irritation, you should urge the operation. I have mentioned the fact that in cases of disease of the elbow-joint one bone is often more affected with the disease than the others, and therefore it will be sometimes necessary to remove a greater portion of the one bone than of the others. In the preparation which I show you the disease principally affected the lower end of the humerus, and a greater amount of this bone was here taken away than is generally necessary; but, in order to compensate for this, a smaller portion of the radius and ulna, which were

very slightly affected, was taken away. Occasionally the disease affects one of the bones connected with the elbow-joint, close to the articular surface, and yet does not involve the articulation. For instance, I have here a preparation of disease of the ulna, in which the disease (caries) extended over nearly an inch and a half or two inches, but did not affect the articular surfaces of the joint itself. Now, in such a case as this, it is of great consequence to prevent, if possible, the joint becoming implicated; and, therefore, if you meet with a case of disease of the ends of one of the bones, with suppuration, it will be a proper proceeding to endeavour to remove the diseased portions of bone as early as possible without interfering with the joint. Should this not be possible, an excision may be performed, and the diseased bone at the same time excised. This was done in the case from which this preparation was removed. Nearly three inch of the ulna, and a smaller amount of the radius and humerus, were cut off.

The third class of cases I would refer to is the class of ankyloses. Excision of the elbow-joint may be required for the relief either of osseous ankylosis or fibrous ankylosis, or of a mixture of both. These ankyloses may be the result of disease or of an old-standing injury. A common class of cases resulting from injury is that in which a dislocation has been unrecognised and unreduced, and the displaced bone acts as the cause of ankylosis or stiffness of the joint. Another class of cases of the same kind is that in which a fracture has been followed by displacement of the bones, which have become fixed in an improper position. You do not perform excision of the joint in every case of ankylosis, but there are two questions I should advise you to consider in connection with this class of cases. The first question is: Is the condition of the elbow, and is the condition of the arm, such that the latter is useful to the patient in his or her occupation or in his or her rank in life? If this question can be decided in the affirmative, then excision is not required. The second question that you should consider is this: Supposing excision is performed, is the arm likely to be useful? Because if there is atrophy, or contraction, or any other condition of the other parts of the arm which will render the arm, after excision, useless, or nearly so, then there is no advantage in performing it. If, however, the other textures of the arm are in good condition, and the arm in its present position is inconvenient to the patient, then such a case is a proper one for operation.

In judging of cases of ankylosis of the elbow-joint—and this remark applies to other joints—you must be careful to determine that the ankylosis depends on causes inside the joint. There may be contraction of the muscles or other

textures outside the joint, stiffening it and making it appear as if there were what is called true ankylosis—that is, ankylosis inside the joint. In any doubtful case, you will get great aid in determining the matter by putting the patient under chloroform, in which condition the muscles are relaxed, and you are better able to judge whether the stiffness depends on true ankylosis or on contraction of the textures outside.

There is another class, which is a class of greater rarity. It is the condition in which there is a large sore from any cause situated over the elbow-joint; and, owing to the size of this sore, and to the fact that you have bone beneath it, complete contraction is prevented, so that it cannot heal. In this case the excision of the elbow-joint will do two things: it will, by removing a certain amount of bone, allow the sore to contract and heal, and at the same time allow the patient to recover with movement of the joint. I have performed two or three of these operations very satisfactorily. In one instance I excised the elbow-joint for a somewhat different cause, which I may include in this class. It was a case where there had been very extensive destruction of the soft textures, and a dense cicatrix had formed, which had drawn up the forearm to the arm, and so fixed the joint. It was a condition of old standing, and so did not admit of stretching; and therefore, in order to relieve the contraction and give mobility, I excised the elbow-joint, and the result was excellent.

There is another class in which the operation of excision may be performed. I have never myself performed it, but a case might occur in which a tumour growing from the bone, from the lower end of the humerus, or upper end of the radius or ulna, admitted of being removed by taking away a portion of the bone from which it grew. In such a case, in order to get a movable joint, it would probably be best to perform excision—that is, to cut away not only the bone from which the tumour grew, but also more or less of the articular ends of the other bones of the joint, so as to ensure movement in it.

A question in connection with excision will sometimes arise in cases which are complicated by diseases in other parts. You may meet, for instance, with a case of disease affecting both elbow-joints, and both elbow-joints have been excised successfully. Again, you may meet with cases of disease affecting other joints. I have brought here a woman having the complication of caries of some of the bones of the spine. The principle which should guide you in these cases is to endeavour to find out which disease is causing most constitutional irritation, and if it is possible—say in the case of the complication of disease in the knee-joint—to remove the disease of the larger joint, either by amputation or excision in the first instance,

and then, when the patient has recovered from the first operation, to act upon the elbow. In this way, I think I mentioned before, I once treated a boy who was very weak and suffering from disease in the knee and elbow-joints. The first disease was the more serious, and I accordingly performed amputation of the thigh, and as soon as the boy recovered, I performed excision of the elbow; and he is now going about a strong, healthy young man. Should the complication be non-removable, as in the case of caries of the spine, excision of the elbow should be performed only if the spinal or other disease is in a quiescent state, and if the elbow disease is causing constitutional mischief or irritation.

We pass now to the operation itself, and we shall best consider the operation of excision under certain heads.

1. What is the best incision? The best incision is one made along the posterior aspect of the joint, a little to the inner side of the middle line. In the adult an incision of about five inches long is usually sufficient, but you never can do any harm by making it longer in the superficial parts, because it is of great consequence to have the deeper parts thoroughly exposed. In cases where there is a wound or sinus, or sore, it is convenient to include that in the incision. For instance, if on the lateral aspect you have such a wound or sinus, you may sometimes modify your incision by taking a curve so as to include the sinus or wound. If this be upon the anterior aspect of the joint, you must not of course include it. The great advantage of the straight incision is that it allows you to split the attachments of the muscles, more especially that of the triceps. If you divide the muscles transversely, the result afterwards is not so satisfactory as regards the strength of the arm.

2. The textures to be avoided. Now, the principal textures to be avoided in excising the elbow are—the ulnar nerve, as it lies over the inner condyle, and the brachial vessels in front. The best way to avoid these structures is to make your incision in the first instance quite down to the bone, and then in your dissection to keep the point and edge of your knife close to the bone, taking care to divide all the muscles where inserted in the longitudinal direction, and as close to the bone as possible. In fact, it is a good rule to separate and cut away a piece of the periosteum along with the muscle. The structure which is most likely to be injured is, as you know, the ulnar nerve. Of course, in ordinary cases, if you can see it, and draw it aside, you prevent any risk of injury. But sometimes, especially in cases of extensive synovial disease, you will find it impossible to see it, and therefore you must keep your knife close to the bone, and turn the parts aside, and if you do this carefully

there is very little risk. But supposing it to be injured by accident during the operation, or destroyed in the progress of disease, you should search for the divided ends, and stitch them together with catgut. The fact that the nerve is injured, or has been divided in the operation, does not necessarily interfere with the usefulness of the arm, and is not sufficient to make you perform amputation. Then, as to the periosteum. It has been proposed, as many of you know, to perform the operation subperiosteally, as it is termed. In regard to this operation, I would say that it is quite right, in cases where you require to remove a very large amount of bone, to preserve as much as possible of the periosteum. But in an ordinary case, I would not advise you to retain the periosteum, except at the points where the muscles are cut away; because if you leave it the growth of new bone is generally too excessive, and you do not obtain a thoroughly movable joint.

3. How much bone is to be removed? If you are operating in a case of injury, you must remove the whole of the injured bone, and, if you have to remove a large amount of one bone, you must endeavour to compensate for this and only take away a small portion of the other bones, or none at all in some cases. If you are operating in a case of disease where one bone is more diseased than the other, you must compensate by taking away less of those bones which are not diseased. I advise you to act upon this principle. Having freely exposed the bones with the incision already mentioned, you examine the amount of injury or disease, or the condition of the ankylosis, and take away as much as necessary to remove the injured or diseased portion, or, in the case of ankylosis, to cause a good gap between the ends of the bones. When you have done this, you should hold the arm in the straight position, and determine that there is an interval of at least an inch and a half between the ends of the bones of the arm and forearm. If you have not that amount of separation, you should take a slice off one or other of the ends of the bones, as may be most convenient. In addition to this you should determine that the arm moves freely, that there is no locking, and that, in fact, you have taken away sufficient to allow of proper mobility. In some cases, where the disease is extensive, you may find that there is a wider interval. There may be two inches, or even three inches, but you should not have less than one inch and a half. In some cases of ankylosis, my colleague, Dr. Watson, has suggested an operation, by means of which he removes through one lateral incision the lower end of the humerus and its condyles, without interfering with the radius and ulna, so as to preserve as much as possible the attachment of the triceps and other muscles. I have done a similar operation by means of two

lateral incisions, and I think that the two lateral incisions give you more room, and you expose the bone more readily. Note this therefore, that in certain cases of ankylosis you may get an excellent result by one or two lateral incisions and by removing only a sufficient amount of the end of the humerus. In connection with the removal of the bone, a point to be attended to is that in cases of ankylosis you should always ascertain that the head of the radius is movable, and not fixed to the ulna. If it is fixed, it interferes with pronation and supination, and the connection between the two bones should be divided with the bone-forceps or saw.

4. The after-treatment. When the excision has been performed, the arm should be placed in a position midway between flexion of the elbow at right angles and extension. If you like you may place the arm upon an angular splint of some kind, so as to keep it steady. But in this school our custom is to adjust the limb upon a pillow only. We have next the important point of movement to be attended to. As soon as the wound is healed, or nearly so, you should begin carefully to make flexion, extension, pronation, and supination; but, even if the wound is not healed, you should not delay beyond a fortnight these movements. These movements should be carried on for a considerable time—for months it may be, until the arm becomes stronger. It will frequently happen that for several months the arm is weak after excision—in fact, until the divided muscles acquire their new attachments and become shortened. If they continue weak after this time you will find that friction and the application of galvanism are often useful in strengthening the arm. It will sometimes occur that you have too great mobility after this operation. This may be owing to the fact that too much bone has been removed, or that the new attachments of the muscles have not taken place satisfactorily, or that some of the muscles have become atrophied. In such cases, if friction and galvanism do not do good, some little mechanical support will often be useful to steady the elbow, and will cause the arm to be very useful. Should there be any tendency for the muscles or other soft textures to contract and interfere with proper mobility of the arm, extension by means of a weight will be found useful in counteracting it. In one case of the kind lately in the wards this treatment was found to be most successful.

The question of re-excision may require to be considered. Thus, re-excision may be required for two conditions—either for ankylosis, which has resulted from the former operation, as in the case of the boy upon whom we operated lately; or it may be required for an extension or return of disease. In both cases you must carry out the operation upon exactly the same

principles as in any ordinary excision performed for the first time.—*Lancet*, Feb. 22, 1879, p. 256.

45.—AN IMPROVED METHOD OF TREATING CLUB-FOOT.

By Dr. ALEXANDER OGSTON, Surgeon to the Aberdeen Royal Infirmary.

The treatment of congenital club-foot (*Talipes Equino-varus*) is one of the most difficult, tedious, and disappointing undertakings in the whole range of surgery.

One great truth, often overlooked, is at the foundation of most of the disappointments experienced. The pernicious doctrine, that ordinary congenital club-foot depends on paralysis or contraction of certain muscles and fasciæ, is still tolerated in the profession, and believed in by the laity. Consequently, a cure, or at least a marked improvement is too confidently expected to result from the operation of subcutaneous division of these structures; and tenotomy occupies the most prominent place in the minds of those interested in the progress of the case. But, in truth, division of resisting structures is one of the smallest and least weighty factors.

If we consider for a moment the true pathology of club-foot, we find ourselves in a better position to estimate the difficulties to be overcome and to give a correct prognosis as to the time and trouble that its cure necessitates. In an infant born with the deformity there exists, properly speaking, neither paralysis nor contraction. The whole limb is distorted, and every structure in it contributes its relative share towards producing the deformity. The skin of the foot and leg, if taken off and stuffed, will assume the clubbed form. The muscles, the tendons, the fasciæ, the very vessels and nerves, are all altered in length to suit the deformed foot and leg, but no trace of disease, nor of paralysis or contraction, exists in them. If we were to take away all the muscles, fasciæ, vessels, and nerves, the bones and ligaments would still exhibit the distorted shape characteristic of the disease; and if, after removing all the ligaments, we were to articulate to each other the cartilaginous bones that remained, we could do so only in the deformed position. Hence it has to be borne in mind that *every structure in the limb, without any exception, contributes its share towards keeping up the malformation, and has to receive its share of the treatment.* Certain it is that some tissues, especially the tendo Achillis, require to be cut by the surgeon, because they, from their powerful structure, resist any other means at his command to restore them to the length they must ere have the limb can be brought into a normal posture; but their division is no essential part of the proper plan of cure, and is resorted to only as a convenience, as an

adjunct, often a very necessary one, to the treatment. The treatment proper consists in fulfilling the indication of gradually bringing the limb into the right form, and adapting the means of cure so as to fulfil the indication of a gradual moulding of bones, ligaments, muscles, fasciæ, nerves, vessels, and skin, into a form that shall approximate, as far as possible, to that of a sound leg and foot. Whosoever treats club-foot on any other plan treats it mistakenly and unscientifically.

Putting aside the question of tenotomy, which is simply an adjunct to all methods of treatment, it may be correctly said that the cure can be obtained only by a gradual rectification of the position of the limb. At that, accordingly, all proper methods aim, and some attain it in one way, others in another, but all, without exception, have their weak points and drawbacks.

Well-constructed orthopædic apparatus is of very great value, and can achieve signal triumphs in this disease. But it has its dark as well as its bright side. Its expense puts it beyond the reach of the poorer classes and the poorer institutions to which they bring their deformed children.

Gradual rectification by plaster-of-Paris bandages—a method we owe to our German brethren—is capable of simplifying and cheapening, to an important extent, the cure of the disease, and putting a really reliable and satisfactory method within the reach of even the poorest. I have employed it for a considerable time, and have been led to make some modifications in the manner of employing it that seem to me to add greatly to its simplicity and success.

This paper, therefore, is designed to bring before the profession the mode of employing it which, I believe, amongst the poor at least, deserves to take the preference over all other methods of treatment.

Inasmuch as all structures of the foot are concerned in the production of club-foot, it stands to reason that the earlier the cure is undertaken after the birth of the child, the softer and less rigid will they be, and the easier will be the cure. But it is not wise to commence the cure in the first few months of life. The child at that epoch requires frequent bathing, has not acquired habits of cleanliness, and perhaps has a more vulnerable skin, so that it is not easy to keep the plaster appliances clean and dry. The best age to commence treatment is from six to nine months, at which time it will be found that the tissues are not appreciably more firm or resistant than at birth, while there is, on the other hand, the great advantage that by the time the foot is rectified the child will be beginning to walk, and the movements of standing and progression will materially aid in maintaining the improved position.

The treatment by plaster bandaging is exercised on the principle of putting the foot into as good a position as it can be made to assume, fixing it by plaster in that position till it has become natural to it; then taking off the plaster, rectifying the position still more, and again fixing it in the improved position, and so on until the foot is straight. The plan of treatment recommended in this paper has nothing new in principle, since it is merely a carrying out of this idea. But it is the way in which it is carried out that is claimed as being advantageous. It is usually recommended by those who employ the plaster treatment, to place the sole of the foot against a flat surface, press it as straight as possible, then apply the plaster bandages and allow them to set; or to pull the foot into as good a position as may be, and hold it firm until the bandages have been applied and become hard. Both these plans I have tried and given up as not sufficiently answering their intended purpose, and have adopted that which is now about to be described as better suited to effect what is desired.

Before entering on a minute description of the treatment which yields such good results, there are *three* preliminary subjects that require a short consideration.

In the *first* place, I would point out, that, even in an infant under chloroform, the clubbed-foot, when moved by the hand or other means into the best position it can be made to assume, resumes immediately on the removal of the force its original malposition, because the tissues spring back into the place that has become habitual to them. But it is possible, by a few minutes of firm manipulation, to tire out or exhaust the resiliency of the structures, so that the foot does not spring back—or rather has less tendency to spring back—but remains on the contrary in a considerably improved posture. For manipulating so as to effect this there is nothing equal to the hand of the surgeon, by which the amount of resistance, and the direction of the point of resistance, can be so accurately gauged and overcome. If the clubbed-foot of a child under chloroform be grasped by the two hands of the operator and steadily and firmly unfolded, the projecting points which ought not to project (such as the calcaneocuboid joint) being firmly pushed inwards by the thumbs, and the toes and front part of the foot unfolded and coaxed outwards by the fingers, a certain improvement of the position is first obtained; then strong resistance is met with in the attempt at still further rectification, as the bones, ligaments, and fasciæ, all the firmer structures in fact, refuse further to give way. But if, disregarding their resistance, the force be continued, and even augmented, not by fits and starts, but steadily and inexorably, not with reckless force, but with a

watchful yet determined persistence in the proper direction, the foot is brought into a position much more nearly normal than might have been considered possible; the bones, ligaments, and other resisting tissues, thanks to their yielding properties at so early an age, are moulded or forced into a comparatively normal situation without any signs of rending or fracture being observable, and their powers of resistance seemingly exhausted, remain in the new position with so little tendency to spring back, that the slightest support enables them to be kept there until the plaster bandages have been applied. Ten minutes steady moulding in this manner are more than sufficient in every case to obtain in a child of nine or twelve months of age a most pronounced and decided amelioration of the pre-existing deformities. It is scarcely needful to say, that the force must never be so wildly and recklessly applied as to produce fracture or dislocation, nor continued to too great an extent in the endeavour to obtain too much at one sitting, but that a decided rectification once obtained, further attempts should be reserved for the next sitting. The merest minimum of practice is requisite to enable the surgeon to feel how much can safely be gained at each sitting.

This force, so applied, must evidently act by overstretching the soft parts, and probably by slight bending or even infraction (similar to greenstick fracture) of the bones, but it is never followed by any swelling, inflammation, extravasation, or other marked reaction, and does not pain the child after it has awakened from the anæsthesia. I may add that fracture or dislocation is very difficult to produce in so young a child, and need never be dreaded if even reasonable caution be preserved.

In the *second* place, I would add my testimony to the truth of that which has been already well and sufficiently pointed out by others, that the foot must be unfolded before the heel is brought down. It is not quite correct to say that club-foot is a combination of the equinus position, where the heel is drawn up, with the varus position, where the foot is inverted, and that the adjective equino-varus expresses the deformity. The deformity is far too complicated to be thus satisfactorily defined, for it affects even the knee-joint, appears markedly in the leg, and reaches in the foot such a degree of complicated twisting that no word or name can adequately express the state of affairs. Still, on the whole, so far as concerns the foot and ankle alone, the term "equino-varus" is *an approximation* to the truth, and, so far as the treatment goes, it is well to retain the term, helping as it does to indicate that there are two main sets of deformities to be overcome.

In the treatment, the foot must always be unfolded and the varus position rectified, before by the bringing down of the heel

the equinus position is interfered with, for the fixation of the heel by the tendo Achillis is an immense assistance, giving an additional prop to work on in unfolding the anterior part of the foot.

In the *third* place, I would remark that division of anything save the tendo Achillis is rarely required. The fashion of promiscuous division of all the main tendons around the ankle cannot be too strongly condemned. It must be remembered that such tendons as possess synovial sheaths cannot be divided without the certainty, or next to certainty, that they will never reunite,—a fact often experimentally proved and never refuted, and that is yet not sufficiently appreciated or known. Although a foot may serve passably well for locomotion after division of a number of its tendons, that is no reason for running a muck against them, under the delusion that they are the offending agents in the disease. It is quite an erroneous and unjustifiable line of treatment to divide the peronei, the tibialis anticus, the tibialis posticus, or the flexor longus digitorum, each of which possesses a synovial sheath around its tendon. Their division is never necessary, is of no assistance in the treatment, and is merely a relic of the time, now gone by, when the pathology of the disease was thought to consist of paralysis of some muscles and contraction of others. In trying to cure the deformed foot it is quite a mistake to divide tendons according to any fixed plan or rule; the only wise course is to wait until, during the progress of the foot towards rectification, it becomes evident that some structure is offering a real and invincible resistance to the means employed, and then of course that structure, whatever it be, must be divided. It will invariably be found that in infants the tendo Achillis is the only tendon requiring division, and that at the very utmost in addition to this the plantar fascia may now and then have to be cut in the way recommended by Buchanan of Glasgow.

Having now exhausted the preliminary considerations necessary to prevent any misunderstanding, we come to describe the procedure which it is my object to recommend.

The child, we will suppose, has arrived at about the age of six or nine months, and the parents have had clearly explained to them the possibilities and the difficulties of the case.

The child has all clothing removed from its lower limbs, and is laid upon a small table of the ordinary height, so that the operator can finish his work without the fatigue of prolonged and considerable stooping. A pillow is placed under its head, and chloroform is administered. Children bear chloroform well, and its administration is most desirable, as it renders the limbs passive and quiet during the rectification and bandaging. The chloroform need not be deeply given, but it must be administered

sufficiently to prevent the child moving during the application of the bandages. The operator grasps, say, the right foot with both hands, back upwards, holding the ankle joint and heel in his left hand, and the front part of the foot in his right hand, with both thumbs pressing on the outer edge of the sole, opposite Chopart's articulation, just below the thin scar-like patch of skin usually present on the most prominent part of the dorsum of the foot. Maintaining steady pressure with his thumbs, he now strives by a deliberate, gradually and cautiously increased force, exerted by his hand, to abduct, evert, and pronate the foot, so as to bring it into as natural a position as possible, but without any attempt whatever to bring down the heel. At first the foot yields readily, but soon ceases to move as desired, and the operator becomes conscious that greater force will be needed to effect his object. Renewing his grasp, if this has been rendered insecure by the improved position of the foot and wrinkling of the loose skin on the outer part of the dorsum of the foot, he continues his task by applying, with the same steadiness and caution, an increased force, in exactly the same manner as before. The force is slowly increased, with every now and then, if there seems to be any danger of fracturing the bones, intermissions of half a minute or so, during which it is merely kept up, and not further increased, until the resistance of the foot being by degrees overcome, it unfolds from its perverse position, and has not the same strong tendency as formerly to spring back again. After as much advance has been made as seems prudent, the hands are removed, and the foot, if the manipulation has been well managed, lies passive and improved in posture, flaccid, and with considerably diminished tendency to return to its old vicious shape. There are no marks of its having suffered from the proceeding; there are no signs of fracture, contusion, or even abrasion, but the foot appears simply more normal in shape.

When this has been achieved the operator next proceeds to fix it with plaster-of-Paris in its new posture. It is bandaged from the roots of the toes to the insertion of the ligamentum patellæ with a single thickness of flannel bandage, one and a half inches in breadth and four feet long.

To retain the foot in the position most nearly approaching that of the normal foot, two strips of adhesive lead paster are used, each of them eighteen inches long by one-third of an inch broad. One of these is made into a loop, applied to the foot over the ball of the great toe, and, guided by the hand of an assistant, pulls the foot outwards, or rather retains it by moderate traction in the favourable posture gained by the manipulation already described. The second strip is likewise made into a loop, passed round the foot over the ankle and heel

at the exact spot that seems most advantageous in each case, and forms a resisting force to the traction exercised by the former loop, by drawing the ankle inwards, or rather preventing its being drawn outwards. The assistant who holds these two stands on the right side of the pelvis of the patient with his face to the child's feet, and uses the toe strap with his right hand and the ankle strap with his left hand. In this manner he avoids interference with the operator, who now proceeds to put on the plaster bandages. A strip of "book muslin," one and three-quarter inches broad and five feet long, with plaster-of-Paris well rubbed into its meshes and loosely rolled up, is dipped for a few seconds into water until it is well soaked, then wrung firmly to expel the superfluous water, and wound round the foot and leg, beginning at the toes. It should not be drawn tight, but simply laid on outside the flannel bandage and loops of strapping where the latter are in contact with the limb, and it should be brought once or twice between the limbs of the straps where they leave the foot to proceed to the hands of the assistant holding them. The toes should be left uncovered. After the plaster bandage is in position, about a heaped breakfast-cupful of dry plaster-of-Paris is made into thin, nearly fluid, paste by adding the required quantity of water and stirring with a spoon, a common iron kitchen spoon serving this purpose best. With the spoon the paste is spread on the foot and leg, first along their upper surfaces, then along their sides, and finally down the back of the leg, over the heel, and on the sole to the roots of the toes, care being taken to insert a sufficient quantity between the limbs of the two straps and carefully spread it out there, so that the bandage may not be weak at these points. The layer of plaster thus applied to the foot and limb should be a quarter of an inch thick, not exceeding three-eighths of an inch at most. It need not be very carefully smoothed on its surface by the spoon or hands of the operator; it is better to lay on outside it without any delay another muslin plaster bandage, which renders the exterior smooth and neat, and adds greatly to its strength and power of resisting injury. For this purpose, while the plaster is being laid on the limb with a spoon, another muslin bandage, similarly prepared to the first, should be soaking in water, and as soon as the plaster is spread on the limb, and while it is yet soft, the second bandage is wrung out and laid on outside the plaster layer in the same manner as the former one had been applied, with the same care to bring it once or twice between the limbs of the loops of strapping. When this last bandage has been put on, any plaster paste remaining over may be used to rub on its exterior and make everything smooth and neat. By the time all this has been done the plaster will have begun to set, and as soon as it is firm

enough not to yield or crack when the straps are relaxed, these should have their ends cut off close to the plaster, and the places where they have emerged be smoothed over with a touch of the plaster paste.

The limb is now finished, the toes alone remaining uncovered, so that they may show by their temperature and colour whether the bandage has been by accident too tightly applied around the limb. If reasonable care is taken not to pull the inner muslin bandage too tight, this occurrence never happens, and the toes remain pink and warm. Should they be slightly bluish and cold, the bandage may still be left on, as that is a condition that will in all probability pass off in a few hours and cause neither pain nor trouble. But if they be found deep purple and quite cold, the bandage is certainly very badly applied, and must come off at once. I have never once, however, seen a bandage require to be removed for this or any other similar cause.

The other foot, the left one in the case we have supposed, is next treated in a precisely similar manner, the hands of the operator who manipulates and of the assistant who holds the straps, being of course reversed, and the straps being also reversed, as is self-evidently necessary.

The whole of the above treatment can be readily carried out in half an hour, or, with a little practice, in even less time. The chloroform is stopped, and the child, when it awakes, shows signs of neither pain nor uneasiness, so soothing is the firm, equable support of the plaster-of-Paris bandage. I have never seen the treatment followed by more than a very slight uneasiness of the child for the first twenty-four hours, and that was probably due to the effects of the chloroform, or to the feeling of weight of the bandages.

The plaster bandages, if well applied, will easily last for six weeks, and for that period they should be left undisturbed. So far as I have been able to judge, no advantage is to be gained by renewing them at shorter intervals. The foot requires some time to become habituated to its rectified position, and, active as the processes of life and repair are in infants, they seem to need a period of about this duration to make the new position normal and habitual to the foot. The parents are therefore instructed to remove the plaster bandages two days before the expiry of the six weeks. They are best removed by cutting or scraping through them with a stout knife, and as their removal requires a good deal of time and trouble, it is best that the parents be instructed to undertake it themselves. The limb ought then to be well washed to remove the rough epidermis that has collected on the skin; and, when seen on the expiry of the six weeks, the foot, having been free for two days, shows by the position it

has assumed the degree of improvement really attained by the treatment. A few slight abrasions, not amounting to complete removal of the epidermis, are generally seen in the form of red lines about the ankle. These offer no contra-indication to a renewal of the treatment. The child is again anæsthetised, and, by the same manipulation and bandages, the foot is still further rectified. Every six weeks a fresh rectification is obtained until the foot has lost all tendency to adduction and inversion, and even become somewhat abducted at Chopart's joint.

It is absolutely necessary not to begin to bring down the heel, *i.e.*, cure the equinus position, until the varus position has completely gone, and even been replaced, as stated above, by a slight abduction. For the foot will in time again become a little abducted, hence the wisdom of curing the varus deformity as fundamentally as possible. In slighter cases three or four bandagings suffice to effect this; in the worst cases six or eight may be required.

Once the varus position has disappeared the tendo Achillis must be cut. It is no use trying to avoid its division. The heel will not come down without it. It is indeed easy by the plaster treatment to produce dorsal flexion of the foot and bring the toes up, but this is effected by a yielding of Chopart's joint, the heel remaining unchanged in position, and the sole becoming convex instead of concave. Hence, in commencing the second part of the treatment, the cure of the equinus position, it is wise, at the very outset, to divide the tendo Achillis. On the subcutaneous division of this structure I have nothing special to say, save in favour of effecting reposition of the foot immediately after it, as saving time and being unattended with any disadvantage.

Accordingly, in undertaking the second part of the cure of club-foot, the correction of the equinus position, the first act is to perform, under chloroform, tenotomy of the tendo Achillis in the usual way, then after having washed the leg with carbolic water, to apply a pad of antiseptic gauze over the small wound, and retain it there with the hand during the manipulations that immediately follow. There is no harm whatever in rectifying the plantar flexion of the foot at the ankle-joint to a considerable extent immediately the tendon is cut, and the operator should therefore proceed at once with the following manœuvre. In the case of the right foot the leg is grasped just below its middle by the surgeon's left hand, back upwards, the fingers of this hand retaining the gauze pad in position over the tenotomy wound. The palm of the right hand is placed against the sole of the foot, the fingers and thumb closing on its dorsal surface, and the toes are steadily

raised, so as to produce dorsal flexion of the foot, until the resisting structures are tired out and yield, just as in the forcible rectification of the varus posture. It is well, however, not to press matters too far in the sitting of which the tenotomy forms the commencement, but to wait for next sitting to wield the manipulation with its full power. After reasonable rectification has been obtained, a flannel bandage is applied as before, and serves to retain the gauze pad over the tenotomy wound, and the limb is fixed in the improved posture so that the plaster may be put on. This is done as follows:—

An assistant, standing on the right side of the child, obtains complete command over its knee-joint by placing the palm of his right hand on its outer side, and grasping the articulation with his thumb closed over the patella, and the fingers crossing behind over the popliteal space, and resting on the head of the tibia behind so as to push it forwards, thus keeping the extremity firmly extended at the knee-joint. Only a single loop of plaster is thus required, and is crossed over the sole of the foot just at the balls of the toes, while its ends, meeting together above the dorsum of the foot, are held in the assistant's left hand, and serve to draw the toes and front part of the foot upwards, and fix them in the desired position. The plaster-of-Paris bandages and paste are now put on exactly as before, and the limbs of the plaster loop cut off. The opposite limb is then submitted to the same treatment. When six weeks have elapsed the bandage is removed, the foot is further rectified, a new bandage is put on, and the process is repeated until a good form and position have been acquired by the foot, a thing generally obtainable by at most three or four repetitions of the proceeding. It is then put into a lacing boot and the child is allowed to walk.

Even then, however, it is well to be suspicious of a club-foot, and emphatically to repeat to the parents before dismissing them that tendencies towards relapse are far from improbable, especially in the direction of adduction of the front part of the foot at Chopart's joint. It has been recommended to advise the mother of every child whose treatment for club-foot has been finished, to sit down with the child once every day and carefully flex the front part of the foot upwards on the leg, following this by a firm and decided adduction of the foot at Chopart's joint, so as to aid in keeping the heel down and the toes outwards. This apparently sensible advice I have never found to be productive of any benefit, since few, if any, mothers will use sufficient force to render the proceeding of any real avail. But it should be well impressed on those who have the charge of the children, that they should return to the surgeon on any serious symptoms of relapse setting in. If a relapse

occur, it must be treated in the way already detailed, according as it is the varus or equinus position which is returning. If varus, the manipulations and bandages for that; if equinus, a repetition, if required, of the tenotomy of the tendo Achillis, followed immediately by the manipulations and bandaging for that. But if the cure have been thoroughly completed in the first instance, and no gross error in the management of the child have been subsequently committed by the parents, a relapse is not to be expected.

This concludes the description of the cheap and simple treatment which I would recommend in club-foot. It is, as already stated, not new, for plaster-of-Paris is used by many on a similar plan, and with success. But the manipulative straightening of the foot which I was led to devise while trying the ordinary plans is of great assistance in shortening the treatment and making it more exact and satisfactory. When mentioning above the number of times the bandages have to be renewed, the maximum number that has hitherto been found needful in the worst cases has been stated, but generally speaking, fewer are required.

Nor is this method claimed as being without defects. At present we know of no method free from defects, but that which is here recommended is as free from them as any I know, and is the only one which seems to me able to compete with the treatment by orthopædic apparatus. Moreover, I believe I am speaking within the mark in saying, that the results obtained by it are in no way inferior to those obtained by apparatus in point either of completeness of rectification or of the time required to effect it.

Simple and effectual as it is, it deserves a trial in the hands of those who have to treat clubbed-feet amongst the poorer classes.—*Edinburgh Medical Journal*, Dec. 1878, p. 481.

47.—ON AN EXTENSION APPARATUS FOR TREATMENT OF CERTAIN INJURIES, DISEASES, AND DEFORMITIES OF THE LOWER EXTREMITIES.

By WILLIAM STOKES, Esq., Senior Surgeon to the Richmond Surgical Hospital, Dublin.

[There is a remarkable unanimity of opinion amongst surgical writers as to the almost impossibility of avoiding shortening in cases of oblique fracture of the femur. Some, such as Pott, Dupuytren, and Syme, gave up extension altogether as unsatisfactory and productive of no good result.]

Notwithstanding, however, the teaching and practice of eminent surgeons, the majority of surgical practitioners still tenaciously cling to the principle of extension, from a convic-

tion, which I believe to be well founded, that by adopting it in some form or other the probabilities of obtaining a minimum amount of deformity are increased. In Liston's splint, and in the modifications and alleged improvements of it, there are undoubtedly grave defects, among which may be mentioned the large amount, and necessity for frequent application, of bandages, making extension from the foot and ankle, and also the oblique perineal bandage. As regards the application of bandages, every practical surgeon will, I think, endorse what Professor Syme has remarked in reference to their use. He observes: "Though they look very neat when first applied, they soon require reapplication. The state of the limb can never be ascertained without either raising the limb to take off the bandage, or cutting it up; and in the latter case, the thigh must be raised in order to reapply the bandage." What Malgaigne has remarked in reference to Dessault's splint bears, too, on this point. He says: "One needs only to apply Dessault's splint once to be convinced that that celebrated surgeon was completely deceived both as to the value of the apparatus and the reality of his cures. In less than twenty-four hours, all the bandages become relaxed; and if we tighten them up every day, we are making *repeated* and not *permanent* extension." The application of the extending force from the ankle has also appeared to me to be a disadvantage, for the nearer the force is applied to the situation of the lesion, as Sir A. Cooper has pointed out in speaking of the reduction of the luxations, the greater will be the power over it in reducing any displacement. Lastly, the oblique perineal bandage has obvious disadvantages, such as chafing of the groin, pain, œdema, &c., which are doubtless familiar to every one present.

To devise an appliance which would enable the surgeon to get rid of most, if not all, the disadvantages I have mentioned, has for some time been a subject of consideration to me. As the resistance in the injuries already alluded to is constant, so the extending force should be constant; and this can best be accomplished by a mechanical power which is, at the same time, sufficiently great and incapable of relaxation. The principle of making extension by means of a screw force has always appeared to me to be the correct one, as it acts steadily and gradually, and does not relax. This principle was originally introduced, if I mistake not, by Boyer, subsequently adopted by some of our ingenious and talented American surgical *confrères*, and advocated more recently by my friend Mr. Richardson of Dublin. The appliances in which it is adopted being not more generally in use, may perhaps be accounted for by the fact of their being somewhat complicated in construction, by there being usually an absence of elasticity in the extending

force, and, lastly, by the absolute immobility or fixation of the counterextending perineal pad, which must impede any attempt of the patient to assume the sitting posture.

In this appliance I have endeavoured to correct the deficiencies to which I have alluded, and have done so, I hope, with a fair measure of success. It consists of two narrow parallel side splints: one for the inner and one for the outer side of the limb. These are made to slide one on the other, to enable the surgeon to adjust them so that the apparatus may be suitable either to a child or to an adult. These splints can be fixed by means of square-headed screws, which are governed by a key similar to what tuners employ in tightening the strings of a pianoforte. In the first appliance I made, these splints were made up of several pieces, varying in length and united by brass plates and winged screw-nuts. The arrangement in this appliance, which I have had made for me by the eminent surgical mechanics, Messrs. Weiss and Co., is obviously superior. At the upper end of the inner splint, fixed on a hinge-joint, is a pad, which should be so arranged as to press on the perinæum. The pad being fixed on the hinge-joint facilitates the patient changing from the recumbent to the sitting posture, or *vice versa*. The point of pressure remains the same, whichever position he may for the time prefer. I need hardly dwell on the great and obvious advantages to be derived from the patient being able to change his posture occasionally. The distal ends of the two side splints are attached to what may be called the extending portion of the appliance. It consists of a skeleton box, which is made to slide on the outer parallel splints; and these latter and the skeleton box are governed by the horizontal screw placed at one side of the box. The object of placing the screw at the side is to obviate the liability of the heel of the patient pressing on it, which might occur if the screw were placed in the centre. The distance between the two portions of the splint can be increased or diminished by rotating the screw from right to left, or *vice versa*. To regulate the elevation of the limb, I have affixed a vertical screw. Across the skeleton box are stretched elastic bands, and on these the foot and lower part of the leg rest. A pad and piece of scored splintwood should be placed on the anterior aspect of the thigh, and kept in its place by a strap or belt surrounding the splint and thigh. The two free ends of a long and broad strip of plaister are then affixed to each side of the injured limb reaching nearly as high up as the fracture, an India-rubber ring having previously been placed in the loop below the heel. The ring is placed over the hook fixed in the horizontal bar at the lower end of the skeleton box. By rotating the horizontal screw, an amount of extending force can be obtained, which, in

the great majority of cases of oblique fracture, will enable the surgeon effectually to restore the normal length of the limb.

It would require more time than is at my disposal to discuss the particulars of all the cases in which I have used this splint. I may mention that I have tested it in cases of fracture of the shaft of the femur, fractured patella, oblique fracture of the tibia and fibula, of contraction of the knee after previous division of the hamstring tendons, of morbus coxæ, and ulceration of cartilages of the knee-joint. The appliance, therefore, is, I think, applicable to a variety of injuries and diseases where rest, elevation, and extension are specially indicated.

There can be no doubt that the vexed question as to whether union without shortening in cases of oblique fractures of the femur is possible may be answered in the affirmative. It was, in truth, obtained in four out of the seven cases of fracture of the femur in which I have used this splint. It is true that two of these cases occurred in young persons aged respectively ten and thirteen years—periods in which it is comparatively easy to obtain union without shortening. Two of the other cases were of men aged thirty-five and forty respectively, and these (when the patients were convalescent) I brought into the hospital theatre, and invited any of those present who chose to determine for themselves by measurement whether there was any shortening present or not. These measurements were most carefully and accurately made, and in several ways, from the anterior superior spine of the ilium to the upper margin of the patella, then from the same process to the inner malleolus, and, lastly, from the top of the trochanter to the upper margin of the patella and the inner malleolus. In one of these cases so measured there was no difference of opinion as to the absence of shortening; in the other case, a question arose as to whether the injured limb was not slightly longer than the uninjured one. I was unable to satisfy myself of this, but there can be no doubt as to the possibility of such a result being obtained, and it should make the surgeon very cautious not to apply a greater amount of extension than is absolutely necessary to restore the limb to its normal length. In two other cases of fracture of the shaft of the femur there had been shortening previous to the accident—the result of morbus coxæ in early life. Both these cases, however, made excellent recoveries.

The last case of fracture of the shaft of the femur to which I shall allude was one in which the patient was of a restless irritable disposition, and could not or would not endure the extension necessary to maintain the fragments in proper apposition. The shortening in this case was fully three-quarters of an inch.

In the other cases in which I used this splint, and to which I

have already referred; namely, of morbus coxæ, fractured patella (in conjunction with Sanborn's method), contraction of the knee, ulceration of its cartilages, &c., I have had no reason to be dissatisfied with it, but quite the reverse.

The advantages, therefore, which may reasonably be claimed for this appliance are these.

1. It is suitable for the treatment of a variety of injuries and diseases of the lower extremities.

2. It can be adapted to children as well as to adults.

3. The use of roller bandages, and consequent concealment of the parts, can be dispensed with.

4. The power of extension is unlimited.

5. The oblique perineal bandage, so frequently a source of annoyance to the patient and of interference with the circulation of the limb, is dispensed with.

6. The use of the splint does not necessitate the patient remaining in the recumbent posture during the process of union.

7. The patient is unable to relax the extending force.

8. The splint is applicable to either the right or left lower extremity.—*British Medical Journal*, Dec. 14, 1878, p. 871.

48.—ON THE TREATMENT OF KNOCK-KNEE.*

By JOHN CHIENE, Esq., Surgeon to the Edinburgh Royal Infirmary.

The relief of aggravated cases of knock-knee or genu-valgum by operation on the deformed bone has lately occupied the attention of surgeons. Cases of knock-knee, after dividing by tenotomy the biceps tendon and the shortened ilio-femoral band, used to be handed over to the care of mechanics, and very various and ingenious forms of apparatus were used to relieve the deformity. In slight cases these means were often efficacious in arresting further deformity. As a general rule, in bad cases little good was done; and there can be little doubt that during the last few years many cases have been remedied by operation, in which in former years no attempt would have been made to give relief, or if attempted by mechanical appliances, no relief would follow.

Before considering the different methods of operation which have been recommended for the relief of knock-knee, it will be well to consider shortly the pathology of the affection.

Genu-valgum may in rare cases be a congenital deformity. I have never seen such a case. In all cases that I have noted the deformity has appeared after the patient began to walk. The onset of the inbending at the knee-joint is generally to be traced to some weakening cause, either constitutional in its nature as rickets, or to some febrile state such as measles. The

exception to this rule which I have observed is one which I may term "traumatic knock-knee." In it an unreduced dislocation outwards of the patella gave rise necessarily to a change in direction in the axis of the extensors; these muscles, pulling on the tubercle of the tibia at an angle for a period of four years caused a tilting outwards of the tibia, and an in-kneed or knock-kneed condition.

In the normal limb the transverse axis of the knee-joint is horizontal. The femur slopes downwards and inwards; the tibia is vertical; there is therefore in the healthy femur an elongation more or less marked of the internal condyle. There is in the normal limb an angle inwards at the knee-joint. If the patient is weakly from any cause at the time of life when he or she begins to walk, or if weakened during the period of life when the bones are still immature and inclined to bend, then the weight of the body acting on a weakened limb will cause a bending of the limb at its weakest part. The osseous column may give way under the weight; the femur or tibia may bend forwards as in rickets, or outwards as in the bandy-legged child; or if the ligamentous structures of the knee-joint are weak the result will be the disease termed genu-valgum or knock-knee. In consequence of the normal angle inwards at the knee-joint it will be evident that there will be an excessive strain on the internal lateral ligament of the joint during the act of walking. If the ligament is unable to bear the strain, it will be stretched and still further weakened, allowing of an inbending at the knee in excess of the normal angle. Along with this stretching of the internal lateral ligament the pressure will be increased between the external condyle of the femur and the outer condyle of the tibia. As a result there will be an arrest of development of the growth of the external condyle of the femur, and an increased growth of the internal condyle of the same bone. The arrest of growth of the external condyle has an analogue in the shortened foot of the Chinese lady. Mr. Symington carefully described last summer in this Society a very interesting preparation, illustrating the fact that absence of normal pressure encourages increased growth of bone. He showed the bones of an elbow-joint in which there had occurred a dislocation forwards of the radius. This had been unreduced, and as a result, and in consequence of its removal from the opposing surface of the humerus, the radius had grown upwards, its neck being greatly lengthened. Coincident with the stretching of the internal lateral ligament there will be shortening of the external lateral ligament, of the ilio-femoral fascial band, and of the biceps muscle. The change in direction of the tibia throws the weight of the body on the inner edge of the foot, the structures then give way, the patient becomes

splay-footed. That the excessive growth of the internal condyle is only in a downward direction, and that there is no increase in the antero-posterior breadth of the condyle, is evident from the fact that, if in a case of double knock-knee, the patient is laid on his back, and the limbs forcibly bent at the knee-joint, all deformity will disappear, the internal condyles of the femora and the inner malleoli of the tibiæ touch one another, an impossibility in the extended position of the limbs. In other words, in the flexed position the tibiæ are parallel to the longitudinal axis of the body, while in the extended position they are at an angle with the longitudinal axis. The alterations in the normal growth of the condyles, elongation of the internal condyle where pressure is diminished, shortening of the external condyle where pressure is increased, was beautifully shown in the traumatic case to which I have already alluded; in it, undoubtedly, the pressure between the tibia and external condyle was increased, while between the tibia and internal condyle the pressure was diminished. This abnormal condition was kept up for a period of four years, and the result was an aggravated case of knock-knee, which was relieved by operation.

To sum up: 1st, Knock-knee is not a congenital deformity. 2nd, It is the result of weakness of the ligamentous structures around the knee-joint generally, relatively to the weight of the body. 3rd, The internal lateral ligament is not specially weak, although it is stretched in consequence of the deformity. 4th, Knock-knee is an aggravation of the normal angle inwards at the knee-joint. 5th, It is not necessarily associated with rickets. 6th, In rare cases it may be due to a traumatic origin; as, for example, an unreduced dislocation of the patella outwards. 7th, One or both limbs may be affected. With these introductory remarks I may now allude to the different methods that have been adopted by surgeons to cure the deformity. The first operation which I saw was performed by Mr. Annandale; in it he removed a wedge-shaped portion of bone from the condyles. Mr. Lister then operated on two cases, in 1876, by removing a wedge-shaped portion of bone from the lower end of the femur; and I believe in Germany surgeons have improved the deformity by removing a wedge from the tibia, while others have operated on both the femur and the tibia. If the essential feature, as I have tried to show, is elongation of the internal condyle, there can be no doubt that the appearance of the paper of Dr. Ogston, of Aberdeen, in the *Edinburgh Medical Journal* in March 1877, threw a new light on the best method of relieving the deformity. He recognised and utilised the fact that the elongated internal condyle is, as he terms it, the "essence of the deformity," and performed an operation by which, after

making a small opening through which he introduced the saw and separated the condyle from the shaft, and then, by pressing the foot inwards, he displaced the condyle upwards, curing the deformity. In April, 1877, an aggravated case occurred in the hospital, and in it I at first intended to perform Ogston's operation. The objections seemed to be the necessity for opening the knee-joint—a procedure which, if possible, should be avoided; however careful we may be in the antiseptic management of the case, mistakes may occur, putrefactive inflammation may set in, and the risk will be increased. I also feared that in sawing off the condyle the integrity of the crucial ligaments, on which the rigidity of the joint depends, might be interfered with.

If there is elongation of the condyle it will be necessary either to separate it from the shaft, as Ogston does, in order to displace it upwards, or it will be necessary to remove a wedge-shaped portion of bone at its junction with the shaft or from its substance, in order to bend it upwards. Mr. Reeve has lately described an operation in which he passes, as far as I can understand it, a chisel into the substance of the condyle, and *divides* the cancellated tissue down to the cartilages, and then forcibly straightens the limb. I have seen Mr. Annandale operate by this method. The criticism I offer is that, if it is necessary to remedy elongation, either to displace or bend the condyle upwards, that this cannot be done satisfactorily in aggravated cases by Mr. Reeve's method, because no bone is removed, and the condyle is not separated from its connexions. The object that I had in view was, if possible, to displace the condyle upwards: 1st, without opening into the joint; 2nd, without separating the condyle; 3rd, with as little interference as possible with the epiphysis and the crucial ligaments of the joint. I hoped to do this, in young persons at any rate, by removing a wedge from the base of the condyle, and then by bending the narrow neck of bone still attaching the condyle to the shaft to displace the condyle upwards. It is but right to add, that Dr. M'Ewen, of Glasgow, described a method of operation (in the *Lancet*, March 1878) precisely similar to the one I have now to describe. He operated on his first case a few weeks after my first operation. We were evidently working at the same thing with the same object in view at the same time, each ignorant of what the other was doing. I have now operated on six patients, seven limbs, and in only one, a boy aged nineteen, has there been a crack indicating fracture when the limb was straightened; in the others, aged fourteen, five, ten, fourteen, and six years respectively, the bending without fracture that I expected took place. In five the joint was not opened into; in one I cannot be sure that this did not occur,

because in it there was an appearance of an oily fluid in the wound resembling synovia; of this, however, I am by no means confident. The wound healed in all the cases under one month; in none of the cases has there been any stiffness of the knee-joint. From the results of my cases I am of opinion that the operation, as performed by Dr. M'Ewen and myself, is sufficient to restore the condyle to its normal position; in other words, to restore the transverse axis of the knee-joint to its normal horizontal position, namely, at right angles to the long axis of the body. When, however, there is deformity due to rickets, curving of the femur or tibia, then it will be necessary to attack these bones in their shafts, and remove a wedge-shaped portion of bone from the bone or bones affected. The objection I have to this procedure is, that the osseous rigidity of the limb as a whole is interfered with, and the difficulties of the after-treatment will be greatly increased. These operations should therefore, if possible, be avoided; they are not, in my opinion, necessary to counteract the deformity when this is due to elongation of the internal condyle.

It only remains to describe the operation: Find the tubercle on the internal condyle to which the long tendon of the adductor magnus is attached; an incision, two to three inches in length, is made over the tubercle in the long axis of the limb. The incision commences half an inch below the tubercle, and is carried upwards for a sufficient distance. After division of the skin and fascia, the tendon of the adductor is exposed. Pass in front of this tendon between it and the fibres of the vastus internus. The bone covered by periosteum is exposed, and the superior internal articular artery is seen and divided, after passing a double ligature below it and tying the vessel. The periosteum is then divided crucially and turned aside, exposing the bone. With the chisel and mallet a wedge-shaped portion of bone is removed from the base of the condyle immediately above the tubercle of attachment of the adductor magnus. The breadth of the wedge will depend on the amount of the deformity. The long axis of the wedge runs downwards and outwards towards the notch between the condyles. The wedge is at a higher level than the epiphysial line. The apex of the wedge may touch the line. After a sufficient amount of bone is removed the tibia is grasped at its lower extremity, and by pressure inwards the neck of bone attaching the condyle to the femur is bent and the limb straightened. A catgut drain is then stitched to the bottom of the wound to the periosteum, the lips of the wound are brought together by horse-hair, the limb is placed in a box-splint, and a light weight attached to the foot by the extension-pulley to steady the limb. The operation is performed antiseptically.—*Edinburgh Med. Journal*, April 1879, p. 878.

49.—ON ANTISEPTIC OSTEOTOMY IN GENU-VALGUM, GENU-VARUM, AND OTHER OSSEOUS DEFORMITIES.

By Dr. WILLIAM MACEWEN, Surgeon and Lecturer on Clinical Surgery, Glasgow Royal Infirmary.

In the *Lancet* of March 30th, 1878, I published a clinical lecture on Antiseptic Osteotomy, detailing four cases and five operations. In it two distinct methods of operating for the relief of genu-valgum were mentioned, both being practised on the condyloid extremity of the femur, beginning on the inner side. The first described the removal of a V-shaped portion of bone, the second consisted in a simple incision without removal of bone. Subsequent writers, in criticising this paper, have confined their observations solely to the first method of operating, overlooking altogether the second. In one instance a surgeon advances a very similar operation, if not one which is identical to the latter described by me; and though he remarks on the first method of operating, he in no way refers to the other. Partly with a view of elucidating these matters, but chiefly in order to present the experience gained by more extended observation, this paper is dictated.

I have now operated on over forty limbs, and have performed over fifty osteotomies, without a single fatal case. Of these over thirty have been for genu-valgum, the remainder being for genu-varum, rickets, and angular ankylosis of knee-joint.

Genu valgum may thus be divided into two kinds, the one in which the deformity is confined to the soft parts, the other in which it is principally osseous, but which has certain important ligamentous contractions on the outer side of the joint. The former is amenable to treatment by splints, bandage, tenotomy, &c.; the latter can only be dealt with by some procedure which will rectify the osseous deformity and the contracted soft parts.

Since the publication of my first paper on osteotomy for genu-valgum, several methods have been advanced. Lately Jackson, of Wolverhampton, has proposed to divide the soft structures on the outside of the joint, and then to straighten the limb. This procedure would, no doubt, suffice in cases where the internal condyle was not the chief factor in the deformity. But where there is osseous malposition, the mere division of these ligamentous and tendinous parts would fail in rectifying the deformity, unless it were coupled with removal of the internal condyle, or by incision elsewhere.

There are several methods which deal with this osseous deformity. M. Delore, of Lyons, does so by what he terms the *décollement des épiphyses*. He effects his end by *brisement forcé*, or a process of forcible straightening. The knee of the operator, placed against that of the patient, acts as a fulcrum, and the

patient's limb is broken over this. The time required for this *redressement brusque* is from five minutes to half an hour, and when the surgeon is tired he is to be replaced by an assistant. Looking at this operation *à priori*, it appears that the fracture of the bone is a little haphazard, and if so would in some instances produce unsatisfactory results.

On the faith of M. Delore's recommendation I tried the procedure in two instances. In a lad of eighteen years I found it impossible to fracture the bone in the manner described, even with the help of an assistant, and while exercising all the force we could bring to bear on the limb. In the other, a boy of five years, the limbs yielded after considerable force had been applied, and they seemed to give way at or about the epiphyses of the femur. In this way the limbs were made straight, but not so straight as after the operation which I generally practise. After the bones were found to have united, the deformity was still slightly present, and he was instructed to return in a few months to have them rectified by the chisel. I speak of Delore's operation with this limited experience, and without having seen him personally apply it. Had this operation been performed in non-antiseptic days, it would have had a very great advantage over any cutting operation. But with strict antisepticism, the compound fracture or osseous incision is reduced to a simple one, and therefore the incision is, in this respect, quite on a level with the simple fracture. Granting this, then, there is the fracture of the bone at haphazard, which one has in the *redressement brusque*, to be compared with its division with mathematical accuracy afforded by the chisel. Of the two, I clearly prefer the latter.

Next there is the class of osteotomies, which are divisible into two: those which aim at a separation of the internal condyle; and the one which I advanced, the division of the condyloid extremity of the femur. There are various procedures for the removal of the internal condyle. Ogston separates the internal condyle by Adams's saw, and Reeves does so in Ogston's line by the chisel. The saw freely enters the joint, and is liable to deposit in its interior bone-débris and blood. The chisel, when used so as to make *an incision only* in Ogston's line, is also very apt to enter the joint, and post-mortem experience shows that it is extremely difficult to divide the bone down to the articular cartilage without permitting the chisel to pass through that structure, so that in the living body it would take indeed a very precise and delicate adjustment to accomplish this end. Even supposing this delicate manipulation were successful, and the instrument did not enter the joint, the articular cartilage, the elasticity of which is limited, would snap, and does snap, when forcible straighten-

ing is resorted to, and, if the deformity be severe, a considerable hiatus will be left on the articular surface, just as in Ogston's operation. Whether by saw or chisel, the separation of the internal condyle has, besides the mere fact of opening the joint, three objections, which are all the more pronounced if the cases are severe. It produces an irregular articular surface, a sharp projection of bone on the inner side of the knee (caused by the sawn or chiselled surface being pushed upwards), and it leaves untouched the strong ligaments and tendons on the outside of the joint, which help to retain the limb in its deformed position.

It may be said that through time the gap in the articulation will be filled up, and the sharp internal angle will be rounded; both of which I grant, provided they do not produce mischief in the first instance, and I prefer not to court these risks and disadvantages gratuitously, all the more so as they may be avoided by another method.

In considering these two osseous defects, I saw that they might be easily remedied by the removal of a wedge of bone in the same line. By chiselling out an accurate wedge in Ogston's line, the articular cartilage forming the apex, the separated part of the internal condyle may then be folded up on end, when it becomes flush with the shaft; and, no strain being put on the cartilaginous apex, it does not snap, and the joint is not opened. This operation I performed several times post-mortem, and, after satisfying myself as to the facts, the operation was carried out in a case of double knock-knee. This was done on May 10th, 1878, and it gave a satisfactory result, but, on the whole, not such a perfect one as is obtainable from the practice of operating through the condyles. For those who prefer the separation of the internal condyle this operation would in those respects be better than the separation of the condyle by saw or chisel, with its consequent defects. It is, however, like all other operations for the separation of the internal condyle, open to the objection of leaving the strong ligamentous and tendinous structures on the outside of the joint as forces to act in preventing the perfectly straight position being obtained without the additional operation of tenotomy, or forcible rupture of the parts.

These defects are obviated by transverse division through the expanded portion of the shaft immediately above the condyle. In my former paper two distinct methods of osteotomy for genu-valgum were mentioned—the one by simple incision, the other by removal of a wedge. The former was the procedure adopted in my fourth case mentioned in that lecture, and, with few exceptions, it is the one which has been carried out by me since. The operation is conducted in the following

manner. While the limb is extended, a point is taken slightly above the level of the upper margin of the patella; or, as my late house-surgeon, Dr. Symington, shows, a more fixed point would be the uppermost part of the external border of the patellar articular surface, which can easily be felt beneath the skin; a line drawn about half an inch above this would represent the incision in the bone, the chisel being inserted straight across the femur in that line. While the limb is extended, a longitudinal incision is made in front of the tendon of the adductor magnus, the middle of the incision corresponding to the transverse line just spoken of. The length of this incision is slightly greater than the breadth of the largest chisel to be used. The division of the soft parts ought to be completed at one stroke, the scalpel passing at once to the bone. Besides the skin and cellular tissue, only a few fibres of the vastus internus are divided. The chisel is then introduced, and after getting it into position, it is convenient to notch the bone slightly by pressing the instrument with the hand firmly against the bone; in this way a line is drawn on the inner side of the femur as a guide for future applications of the chisel, as the instrument is not so broad as the antero-posterior aspect of the bone. For the same reason, it is convenient to divide in a systematic manner the bone from either the posterior or anterior end, the groove in the femur serving as a guide. When making the incision on the posterior border, it is well to depress the handle of the chisel, so as to cut outwards and slightly forwards. In this way the risk of wounding any of the soft parts is avoided. The femoral incision ought to be effected with as few withdrawals of the instrument from the wound as possible. In making the incision in the bone the limb is generally kept extended, but some think that by flexing the knee the vessels are thrown further away from the seat of operation. Generally, surgeons, in practising this operation for the first time, make the incision through the centre of the shaft very well, but they leave a little portion of the bone in front or behind undivided, when they experience difficulty in fracturing the outer layer. If attention be directed to this, the shaft is easily divided. Before attempting to fracture, the bone ought to be divided for over two-thirds of its thickness, and the smallest chisel may advantageously be carried further, so as to give direction to the fracture. If any attempt is made to fracture when the osseous incision has only penetrated to one-half of the femoral thickness, there is then a risk of the internal condyle being broken off, the fracture running from the incision toward the inter-condyloid notch. If this happened, there would be the same tendency to leave an irregular articular surface as there is in the operation of sawing or levering off.

the internal condyle. If the separation of the internal condyle were carried out in this way, it would have the advantage of offering no risk of the instrument entering the joint, the chisel running parallel to the condyles of the femur. In one case, where this was intentionally carried out, it gave a comparatively good result. I, however, do not recommend this procedure, as among certain disadvantages it has those belonging to the separation of the internal condyle by other methods. The separation of the condyle in this way may be easily avoided by following the above instructions—the division of the femur for more than two-thirds of its thickness before attempting to fracture. In making the incision through the bone, if the deformity be slight, a single chisel may be used, and if so it ought to be made to divide the bone without being withdrawn. If the deformity be greater, two or three instruments of varied thickness may be used, the first being the thickest, the second thinner, and the third the thinnest of all. In this way a wedge-shaped opening is made without removal of bone. If the deformity be still greater, the wedge opening may be enlarged by *gently* levering aside the soft bony tissue. This levering aside is best done after the incision is completed, and with the instrument fully into the aperture. It may be seen that this levering aside would do away with the necessity of using so many instruments, the compression of the bone being substituted for the varying thickness of the chisels. It is thus seen that no bone is removed, and, what is of still greater importance, the wedge-shaped opening is made, on the inner side, of only half the size required for the rectification of the deformity; the other half is obtained from the outer side of the bone. If it be remembered, the incision goes in to fully two-thirds of the femoral transverse thickness, the remainder of the bone is snapped or bent. In so doing, the apex of the osseous incision acts as a fulcrum, and as the bone is bent round so as to fill up the internal wedge, the outer layers of bone are either well stretched or a wedge-shaped hiatus is left. In this way both the outer and the inner side of the bone contribute to the formation of the wedge. If a complete wedge were removed, the apex being at the external layer of bone, then there would be no hiatus left on the outer side. It would, however, be open to two disadvantages: first, it would be difficult to do; secondly, it would be difficult to cut the exact size of the wedge to be removed. On the other hand, if a simple incision is made, the deformity is rectified at the expense of the outer side entirely. This does very well where the deformity is slight, but where severe, it would allow the deformity to be entirely rectified by the outer side, and the wedge so formed on the outer side might be greater than could

be firmly filled up. But by the division of the bone in the way described by me, a very slight wedge being formed on the inner side enables the bone to be fractured more easily than if the bone were simply divided by an extremely narrow and fine chisel; secondly, it divides the wedge between the outer and the inner side, leaving the portions to be taken from the latter to be determined by the amount of straightening necessary to make up for the deformity; thirdly, this small hiatus or rarefaction of the osseous tissue on the outer side will be more quickly and firmly filled up, and the periosteum is retained, though stretched over this slight separation. This would not be the case if the gap made were large, as the periosteum would then tear. I believe also that this method will make a more seemly limb than by taking a large wedge from the inner side, or by forcing out a wide gap on the outer.

Some, in criticising this operation, have expressed a fear about the effects of separating the epiphyses, lest the growth of the bone would be arrested. If this operation were through the epiphyses I would not entertain any apprehensions of this kind, from, among other reasons, the fact that Delore has separated the epiphyses in over two hundred cases without bad result. But the line of incision indicated in my operation is clear of the epiphyses—being above it, through the expanded portion of the lower third, or condyloid extremity of the femur,—and therefore cannot in any way be liable to produce such a result. Again, the line of incision being above the insertion of the external lateral ligament and the popliteus tendon, these structures are moved along with the external condyle, and in no way interfere, but rather help in rectifying the deformity.

This operation, then, does not remove any bone; it neither enters the joint nor interferes with the epiphyses; it rectifies the deformity by simple incision, making a compound fracture, which, under antiseptics, may be considered a simple one. This is the operation which I advanced in my former paper on Antiseptic Osteotomy (in the *Lancet*), and which, after having tried various other methods, I still adhere to.

I now wish to refer incidentally to another procedure which had been tried in some very bad cases, especially in rickety subjects, and which may yet be legitimately practised in exceptional cases. On April 24th, 1878, having divided the femur by a single chisel, I found that, though the limb was nearly straight, it was not quite perfect. I therefore divided the tibia below through the spine. This was found quite sufficient to rectify the remaining deformity. In several of my earlier cases this was carried out, but for a considerable time past I have found the femoral incision above described to be

ample for all purposes, giving perfectly straight limbs, even in markedly deformed cases.

In a paper published on May 25th, 1878, Mr. Barwell relates a case in which he had to divide both tibia and fibula, and proposes these operations for certain cases of knock-knee. I have never found it necessary to divide the fibula in knock-knee, nor have I ever seen a case that required it. The fibula is not articulated to the femur, but to the tibia, and therefore, as a bone, can have nothing to do with the formation of knock-knee. It is a long slender rod, and readily bends to suit any slight opening made in the tibia. Its head, giving attachment to the external lateral ligament, might offer a point of resistance, but its shaft is so easily bent in young subjects as to afford a considerable yielding. Though I can scarcely see the possibility of a case of knock-knee requiring its division, yet, if such did present itself, it would be easy to snap the fibula, at the same time as the tibia is bent upwards after division, thus making a simple fracture of this thin rod, as I do in genu-varum.

With regard to the wounds, they are not more than three quarters of an inch in length, and sometimes less. The wound on the inner side of the femur never requires a ligature; a little pressure with a sponge after the elastic band is removed is sufficient to stop the oozing. Formerly a catgut drain was inserted; now this is dispensed with, and the wound is dressed in the usual antiseptic method. When thoroughly healed the wound presents a faint white line, about half an inch in length.

I have now operated on thirty-three limbs affected with knock-knee, the majority by the femoral incision alone, and in all the results have been highly satisfactory. There have been thirty operations performed by my method by six Glasgow surgeons; and if their figures be added to my own, these present over sixty operations for genu-valgum by this method, without a single untoward result.—*Lancet*, Dec. 28, 1878, p. 911.

50.—REMARKS ON MORBUS COXARIUS.

By Dr. H. MACNAUGHTON JONES, Professor of the Queen's College, Cork; Surgeon to the County and City of Cork Hospital for Diseases of Women and Children.

It is with the view of showing that this disease, as most British surgeons believe, has at times a strictly diathetic source, irrespectively of even the slightest injury, that I bring forward these cases; and as the hip-joint has been excised in three out of the four, and a portion of the femur in the fifth, I thought that the brief notice which I shall give of their history might

elicit an useful discussion at this meeting, and an expression of opinion on the merits, indications, and contraindications of excision of the hip in desperate cases of this formidable affection. Three of the children were of the same family; they were brought up until the last few years in the country, and had never had any ailment nor any serious illness; the parents are both perfectly healthy, in fact robust. The children have all had, from the time I saw them, an anæmic sallow appearance; they are dark complexioned and pallid; there were five other children, and three of these have died, as stated by the parents, of various lung affections, since the removal of the children into the city, and one has recently had an attack of hæmoptysis.

Case 1.—Hannah C., aged 6, admitted January 2nd, 1877, had been unwell, suffering from morbus coxarius in the second stage of the disease. No history of injury of any kind; parents positive on this point. There was a considerable shortening of the limb (two inches). She was suffering also on admission from diarrhoea and vomiting, with violent periodical pain both in the stomach and hip-joint. She had outside one or two attacks of syncope, with some severe epigastric pain, when her friends thought she was dying; these recurred in the hospital. On admission, the child was put under chloroform, and the limb gently extended and kept so with weight and pulley. After a short stay in hospital, the parents took a panic after one of the attacks of syncope and removed her. She suffered such pain at home that she was brought again to the hospital on April 19th, 1877, with symptoms of acute inflammation about the joint and threatening of abscess. This rapidly formed, and the abscess was opened antiseptically shortly afterwards, and a quantity of purulent matter evacuated. The leg was kept from this time under extension, and antiseptic dressings used. Very soon another abscess formed under Poupert's ligament beneath the pubes, which was treated in the same manner as the first. From this time, the child passed through the usual painful course of suppurating hip-joint, with discharges, pain, hectic, occasional diarrhoea, wasting, &c. On September 23rd, 1877, I determined to excise the head of the bone and remove any of the diseased portion. Accordingly I did so, finding the head of the femur completely destroyed, and the shaft softened to about two inches below the trochanter. I gouged away a portion of the acetabulum. The entire operation was performed antiseptically, and the subsequent dressings for some months were antiseptically applied. I did not expect much from this operation, and my fears were confirmed by the subsequent complications, which made the nursing and care of the case extremely difficult. Large pale flabby granulations formed over the wound of an unhealthy strumous nature; the abdomen became

tumid and tympanitic; there were periodical attacks of diarrhœa and hectic; and, to make matters worse, the child got an attack of strumous corneitis and an eczematous eruption over the entire face. However, for the past few months, the wound, with care and by alternative disinfectant dressings, such as carbolised oil, salicylic acid, &c., has put on a healthy appearance, and is now healing. The child is looking well, the appetite is restored, and the limb is kept in a box splint, in which it has lain from the first, after the plaster-of-Paris dressings with extension by weight. Certainly the present condition of the little patient exceeds my utmost expectations.

Case 2.—Mike C., aged 4, brother of this last patient, was admitted on January 12th, 1877; had been ailing for twelve months; now in the third stage of morbus coxarius, *râles* all over right lung, and diarrhœa, with perspirations and hectic. He remained in hospital until the 19th of May, under extension with weight and pulley; he was then so much improved that he was sent for change to the convalescent home. He did not come back to hospital (having been taken from the convalescent home to his own house) until September 1st, when he was admitted again with an abscess on the point of bursting over the trochanter. This was aspirated, and the cavity washed out antiseptically. But from this time he became worse, diarrhœa set in, and he suffered great pain. On December 19th, I operated as in the last case, and the subsequent treatment was the same. For a time, he appeared to do remarkably well, and the wound healed. There have been two or three fresh formations of pus, which have been evacuated. The wound followed much the same course; there was, however, great emaciation, and more tympanites than in the case of the sister. He is still in hospital, and it is difficult to say how the case may terminate. [He has since died of the constitutional complications, nearly eleven months after operation, the wound having fairly healed.]

Case 3.—Mary C., aged 8, sister of these two children, was admitted on May 1st, 1877, with pain in the hip and knee. This pain had rapidly come on with lameness, since she had a slight fall fourteen days previously. There was acute pain in the hip on admission. She had bronchial *râles* over both lungs, was delirious, with a nightly temperature of 102 deg. This girl was treated in the hospital at first with a long splint, subsequently by weight and pulley, with constitutional remedies. The pain left, and she greatly improved; all sign of deformity had disappeared. The father begged to take her out of the hospital after three months' stay in it to send her to friends at the seaside. This I afterwards found was not done, and she was sent back to me on September 1st, her health otherwise fair. She was again treated by extension with weight and

pulley, which I afterwards changed for side-splints; but gradually an abscess formed over the trochanter, which was early evacuated by the aspirator, and reaspirated on refilling. On April 14th, 1878, she was taken home, and there used the long splint, and is, I learn, now doing very well.

Case 4.—E. P., aged $8\frac{1}{2}$, admitted in December 1877, with morbus coxarius, otherwise in fair health; shortening to an extent of one inch and a half. Under chloroform, and without force, the leg readily came down. It was put up then with leather pelvic splint and side splint; subsequently I put him up by extension with weight and pulley. He went on well for some time, when an abscess formed below Poupert's ligament; and, on aspiration, a quantity of purulent matter was withdrawn. This abscess was aspirated on three occasions antiseptically, but refilled after each evacuation. Subsequently, I opened it freely, and a quantity of pus escaped. From this time, he followed the precarious course of the disease; the abscess continued discharging; the health gradually failing; he began to assume the characteristic waxy look which accompanies the later stages of the disease, and the nightly temperature range averaged from 101 deg. to 103 deg. I hesitated, by the parents' wish, to interfere, though pointing out the ultimate prospect of an operation. There was no doubt of extensive bone implication, though at no time was there any albumen in the urine. I found, on probing, general destruction of the joint. Keeping up his strength as fairly as possible, I waited my return in August to interfere, and I was anxious for Dr. Sayre to see this and the next case I record. Another opening now came at the inner side of the thigh by the inner border of the pectineus. Dr. L. H. Sayre, who saw the case with me, agreed as to the necessity for excision, remarking, however, on its unpromising nature. Accordingly, on the 23rd September, I operated for excision of the head of the femur in the usual manner, under the antiseptic method, removing the pieces shown. The joint was completely disorganised, so I gouged away the diseased portion of the acetabulum. There was difficulty in thoroughly carrying out the antiseptic method on account of the inner opening and sinus, which I did not think it prudent to interfere with further than to slit it carefully up and cleanse it with strong carbolic solution. Otherwise, the wound was dealt with in an antiseptic manner. There was little or no discharge from the part, and the only pus which formed came from the old sinus, and that was very slight. The child continued for several months to promise fairly: the wound healed, the tympanitic abdomen diminished, the child's spirits and appetite improved, but the diarrhoea continued. Again, in February 1878, the wound put on an unhealthy appearance.

Grey exuberant granulations sprouted out. A fresh abscess appeared at the inner side; and, though he held on until May 28th, 1878, and the wound again became healthier, he died on this date from the prolonged diarrhœa, which resisted every means used to check it.

Exsection of Portion of Femur for Morbus Coxarius.—This photograph represents a child aged two years and six months, on whom I operated, removing a portion of the great trochanter and shaft of the femur (a wedge-shaped piece) for disease of the bone, which I decided did not involve the joint. I performed this operation antiseptically. The child had been three or four times aspirated previously, and was in wretched health at the time of this operation. He remained in the hospital altogether twenty-five months, being ten months old when admitted, and twenty-one months when this operation was performed. There was complete re-formation of bone, and perfect consolidation. The child can walk, and was able to stand (both legs being of an equal length) before he was discharged from the hospital.

Referring to such cases, Mr. Erichsen says: "In such operations, I have not only successfully removed the greater part of the trochanter, but have even scooped out a portion of the interior of the carious neck of the thigh-bone at its trochanteric end, thus preventing the inevitable disorganisation of the hip-joint which would have resulted if the carious bone had been allowed to remain in close proximity to the articulation. In this operation, the surgeon necessarily comes into very close proximity with the capsule of the joint, and, unless great care be taken, this may be opened, and thus the very mischief induced which the operation is undertaken to prevent."

I should mention that, in the child to whom I have just referred, a fall from a table was traced as a cause for the disease.

The practical features of interest in these cases are the following. Three children of the same family suffer from hip-joint disease; two, as far as could be ascertained, having received no injury; the third, from a very slight cause, immediately exhibited symptoms of the disease. In two out of the three, the disease runs its course, ending in complete destruction of the joint; the third is saved by early recognition of the disease and subsequent aspiration. This certainly would point to a special constitutional and diathetic cause. Excision gave great relief in all the cases in which it was carried out; the one fatal case died of the constitutional complications rather than of the local disease. One thing only was to be regretted, namely, that the operation was not performed earlier, before such extensive disorganisation of the joint occurred; but the difficulty of estimating the extent of the mischief was specially illustrated in these children; this uncertainty rendering the time of inter-

ference an exceedingly difficult matter to decide on. But this taught me that this is an operation to be carefully considered, and not lightly put aside (for the reason before quoted) in any of those desperate cases of morbus coxarius in which disintegration of the joint has happened. It is very simple of performance, the entire difficulty being in the subsequent dressings, and the great attention which the children require. My little patients were watched incessantly day and night by the ladies in our Children's Hospital, and no bedsores or excoriation from first to last were present.

I would put these propositions before the Section.

Morbus coxarius is a disease which in a large number of cases has a purely constitutional or strumous origin, and may occur without any injury, or from so slight a shock to the joint that we cannot, strictly speaking, assign the term traumatic to it.

Aspiration, to be successful, must be performed in the earlier stage of effusion or of pus formation, and hence the importance of detecting the first sense of fluctuation and watching closely the symptoms of approaching abscess in or about the joint.

Though we may be guided by the situation of the sinuses, yet it is extremely difficult by means of the probe to ascertain the extent of the joint implication; and in those cases where sinuses form and suppuration has occurred without accompanying symptoms of waxy degeneration, the propriety of excising the head of the bone should be considered, and the earlier this step is taken, when once the necessity for it is foreshadowed by the urgency of the case, the better.—*British Medical Journal*, April 5, 1879, p. 504.

51.—REMARKS ON RESECTION OF THE TARSAL BONES FOR TALIPES.

By JAMES F. WEST, Esq., Senior Surgeon to the Queen's Hospital, Birmingham.

The lecture on excision of the cuboid bone for exaggerated cases of talipes equino-varus by Mr. Richard Davy, of the Westminster Hospital, which was published in the *British Medical Journal* of April 29th, 1876, together with the abstract of a lecture, also by Mr. Davy, on the same subject, in that *Journal* for December 15th, 1877, and the case of resection of the tarsal bones for congenital talipes equino-varus brought before the Royal Medical and Chirurgical Society on October 10th, 1876, by Mr. Davies-Colley, of Guy's Hospital, having drawn my attention to the possibility of relieving deformity in old and severe cases of talipes, I determined to test the value of operative procedure by bone-resection on the first suitable case that came under my notice.

The intractability and relapsing character of club-foot, notwithstanding division of plantar fascia and tendons, and the expense to poor patients of procuring suitable instruments, as well as the necessity for prolonged surgical treatment, fully justify some attempt at relief by other measures than the ordinary treatment by tenotomy and mechanical appliances.

The procedure which Mr. Davy first adopted was resection of the cuboid alone, and he reported three cases in patients aged respectively 15, 9, and 14 years, where it had answered well. Subsequently (in 1877) he was "ready to defend, not only ablation of the cuboid, but an accurate removal of a wedge-shaped block of the tarsal arch;" such wedge embracing the cuboid, the head of the astragalus, part of the scaphoid, the base of the metatarsal bone, and a chip of the external cuneiform bone. Two cases operated on by him after this manner support the opinion in favour of resection of the tarsus, which Mr. Davy had enunciated.

From the results which ensued in Mr. Davy's cases, also in the one reported by Mr. Davies-Colley, and in the patient whose history I now bring forward, I think surgeons will be justified in resorting to similar operative measures in analogous cases, and in looking upon any patient with club-foot, whatever his age or however great the severity of the deformity, as remediable, if not perfectly curable, by resection of one or more of the bones of the tarsus. The operation may be rendered bloodless, or comparatively so, by the use of Esmarch's bandage, and the antiseptic method of treating such operative cases deprives them of the danger which would otherwise accrue from opening so many joints and synovial sheaths without such precautionary measures.

Tenotomy will remain, as before, the operation for the relief of club-foot in children and infants; but in adults, and in many of the severe forms of talipes, where tenotomy has been resorted to and has failed, resection of one or other of the tarsal bones will be found an useful addition to our catalogue of surgical operations.

The bones at first removed in my patient (whose age was 23) were the cuboid and the astragalus; but, as I found the shape of the foot could not be restored without also taking away the scaphoid, I did not hesitate to resect that bone also. As far as one can judge, after the lapse of ten weeks, the patient is likely to have a useful limb instead of being for life a cripple, and of suffering constant pain from ulceration of the structures on the outer side of the foot, on which she had been obliged from infancy to walk.

So favourable a result in an adult could not possibly have been attained by tenotomy, as the shape of the bones removed,

which are now exhibited along with models of the limb, the one taken before the operation and the other quite recently, forbid the belief that, if they had remained, the foot could have ever been restored to its normal shape and position.

Resection of Tarsal Bones for Talipes Equino-Varus in an Adult.
—Annie G., aged 23, single, was admitted into the Queen's Hospital under the care of Mr. West, on May 13th, 1878. Her family history was good. She was born with her left foot turned inwards. On admission, she was suffering from talipes equino-varus. Her foot had always been painful, and more so in damp weather. Till sixteen weeks ago, she was able to walk about, but since that time she had not been able to bear any weight on it, because of the great pain which exercise produced. The tendo Achillis was contracted, as also were the tendons of the tibialis anticus and tibialis posticus, drawing the foot upwards and inwards so that the outer border of the foot rested on the ground when she tried to walk. The plantar fascia was contracted. The ungual phalanx of the second toe was drawn inwards and overlapped the great toe. The skin on the outer border of the foot was much thicker from walking on it, and there was slight ulceration of the skin at the base of the fifth toe. It was almost impossible to say what was the position of the tarsal bones.

May 19th. After some consideration of the comparative merits of resection and tenotomy in long standing congenital equino-varus, the former was decided upon. The patient being anæsthetised by ether, a semilunar incision was made on the outer side of the foot over the supposed position of the cuboid. After division of the ligaments, that bone was removed with great trouble owing to its altered position and relations. The astragalus was then excised, and the tendons around the ankle forcibly stretched. The scaphoid bone appearing to nullify all my endeavours to get the foot into a good position, was also removed. The skin-flaps were carefully adjusted and retained by sutures, after a drainage-tube had been introduced into the cavity of the tarsus. The operation was conducted under the carbolic spray. The hemorrhage, which was somewhat extensive, was quickly subdued by the pressure of the dressings. An outside splint was applied.

May 20th. The patient had suffered a good deal of pain. On removing the dressing, the wound was not perfectly antiseptic. The foot was in a good position. The patient was well in health; the discharge was free. Lister's treatment was continued.

May 24th. The wound was dressed every second day. The discharge was copious, the wound was healthy, and free from unpleasant odour.

May 30th. The discharge was diminishing; the patient's condition was good.

June 6th. The discharge was much less. The line of incision was granulating freely; the limb was less painful.

June 12th. The patient was going on well, both locally and constitutionally. The foot was placed on an anterior splint and swung in a cradle, to improve its position.

June 19th. The granulations, which were exuberant, were ordered to be touched with sulphate of copper.

June 26th. Antiseptic dressing was discontinued. The foot and leg were ordered to be put up in a plaster-of-Paris case, the patient being placed under chloroform in order to prevent pain while the limb was being put in its natural position. A window was decided to be cut over the wound of incision, so that it might be dressed daily with red lotion.

July 9th. The plaster case was removed, as by its pressure it had caused an abscess on the back of the heel, from which escaped about half an ounce of pus; a drainage-tube was introduced, and stypium applied. The limb was ordered to be laid on a pillow, and no apparatus used for twenty-four hours.

July 10th. A gutta-percha splint was applied to the front of the limb; the dressing was repeated as before.

July 15th. Another small abscess having formed in the outer ankle, it was opened and dressed with stypium; an inside gutta-percha splint was applied.

July 30th. The openings were dressed every day with red lotion and were doing well. The original wound was quite healed by August 5th, and a light starch case was then applied, in which the patient was able to walk a little.

Note.—I beg also to call attention to Mr. E. Lund's paper on the same subject, which was read at the Birmingham meeting of the British Medical Association, and which I ought to have mentioned before, as, being one of the local secretaries of that meeting, I had the opportunity of hearing the paper discussed. Mr. Lund ingeniously removed the astragali in a severe case of talipes equino-varus, where ordinary treatment had failed (see *British Medical Journal*, 1872, vol. ii., page 438). In my case, I believe that mere resection of the astragalus would not have sufficed to relieve the deformity, and I certainly think that Mr. Davy's plan of removing a wedge-shaped portion of the tarsus is more likely to be effectual in similar severe or neglected cases of talipes equino-varus.—*British Medical Journal*, Nov. 2, 1878, p. 657.

52.—ON PAPER LINT.

By JOHN CHIENE, Esq., Surgeon, Edinburgh Royal Infirmary.

During the last three months paper lint has been tried in the wards under my charge in the Edinburgh Infirmary. The first notice of its use with which I am acquainted is to be found in the Philadelphia Medical Times, March 30, 1878. In this paper by Dr. Keen, one of the surgeons in St. Mary's Hospital in Philadelphia, the paper lint is recommended, first on account of great absorbent powers, second on account of its cheapness. The objection to its use is that it tears too easily. Dr. Keen hopes to remedy this by introducing into its substance threads of cotton. The form in which it has been used by me is as boracic paper lint.

Common lint which has been soaked in a boiling saturated solution of boracic acid is hung up to dry; after drying, the water passing off, the lint is loaded with boracic crystals. The lint so prepared is antiseptic, and is the form of antiseptic recommended by Mr. Lister in the treatment of ulcers.

If boracic paper lint can be easily prepared, and if it is equally efficacious, a great saving will be effected. The boracic paper lint is prepared in the laboratory of the Edinburgh Infirmary in the same way as the boracic common lint. At first difficulty was experienced, but these difficulties have been overcome by Mr. Barclay, M.B., C.M., and a very serviceable lint is now made. The paper lint takes up its weight of boracic crystals. If it comes into general use it might possibly be impregnated with the boracic acid during its manufacture; boracic acid being non-volatile, the paper pulp could be made with a saturated solution of boracic acid, and the paper then passed over the drying frames.

Paper lint can be used for almost every purpose that common lint is used. I have made use of it principally in the treatment of ulcers, as recommended by Mr. Lister. The ulcer is thoroughly rubbed with chloride of zinc solution (forty grs. to the ounce), the surrounding skin purified with one to twenty carbolic lotion, a piece of protective the size of the ulcer is then placed on the sore, and then a single or double layer (depending on the amount of the discharge) of boracic paper lint is laid over the protective, overlapping it in every direction. The lint is soaked in the lotion before application. The discharge passes out below the protective into the paper lint. After the first dressing (if this is sufficient to purify the sore), boracic lotion is used as being sufficiently powerful and less irritating than carbolic lotion. In very putrid ulcers it may be necessary to apply the chloride of zinc lotion twice before purification is complete. The frequency of the after dressings will depend on

the amount of the discharge. As far as I am able to judge, paper lint will supersede common lint, not in consequence of its superiority, but in consequence of its cheapness.

We have now a cheap paper dressing which can be impregnated with a non-volatile antiseptic, like boracic acid, *without interfering with the porosity* of the dressing. We have yet to get a paper dressing which will retain a volatile antiseptic like carbolic acid (giving it off at the temperature of the body) and which will still be porous. Dr. Keen speaks of this as a simple matter, but my experience is that it is a matter of some difficulty. In the gauze the carbolic acid is retained by resin; this material renders the paper at once non-porous, and therefore useless in soaking up the discharges. Two years ago paper suggested itself to the writer as a material which might replace the gauze if it could be loaded with carbolic acid and still retain its porosity, the carbolic acid being retained in the paper by some material analogous to the resin in the gauze. Many experiments were made, but no material which could compare with the gauze as a dressing could be obtained. Gelatine retains carbolic acid; it is at the same time soluble in water, and some preparation of paper might be obtained loaded with gelatine and carbolic acid. The American chemists have supplied us with the paper dressing, perhaps they may assist us still further by loading the paper during its manufacture with boracic acid, and by discovering a method of retaining volatile antiseptics in paper without interfering with the porosity of the material.—*Practitioner*, December 1878, p. 406.

53.—ON SOME OF THE SURGICAL USES OF ELASTIC COMPRESSION.

By Dr. J. C. OGILVIE WILL, Surgeon to the Aberdeen Royal Infirmary.

Before entering upon a review of the affections in which elastic compression has been found useful, I shall first direct attention to the nature of the appliances used in carrying out this plan of treatment, and with this view I now pass round for your inspection two bandages somewhat dissimilar in appearance, but which both owe their remedial properties to the presence of the same material—india-rubber—while they also act in a precisely similar manner, the use of either being determined by the presence or absence of certain conditions, of which notice will be taken in due course.

The first bandage is that known as the “Strong elastic bandage,” which Dr. Martin, its inventor, thus describes:—The material of which it is made is what is technically called “pure rubber.” Its length is $10\frac{1}{2}$ feet, width 3 inches, and

thickness of No. 21 of "Stubs's wire gauge." Into one end a piece of strong linen cloth is inserted, and to this is attached a stout double tape 18 inches long. Bandages of greater length and width are occasionally required, but the dimensions given are those of the bandage which will be found most generally applicable. The surface of the bandage is extremely soft and smooth, and its edges perfectly even, so that there is no risk of its tearing, which it would do very readily if the slightest notch were present. If made of the best Para rubber, prepared with the minimum of sulphur and heat needed to effect "curing" of the gum, a bandage will not only last for a very long period, but will be found to improve in appearance as it loses the sulphur employed in its preparation. The durability of the bandage is matter of some moment, as the cost of a single one of the size mentioned is ten shillings.

The other bandage is an elastic web one, which we owe to Professor Lister, who introduced it some years since, and who applies it over the edges of antiseptic dressings to prevent their shifting, a purpose for which, I feel assured, all who have tried it will agree with me in regarding it as simply invaluable. As its name and appearance explain sufficiently well its nature, I need not occupy time in describing it. It can be obtained of any length and width desired—the price is about one shilling per yard.

Having described the bandages, I now pass to their uses and modes of application.

The first class of cases deserving of notice in which Martin's bandage may be used with undoubted advantage is ulcers of the legs, including those dependent upon a varicose condition of the superficial veins, the treatment of which by any means short of prolonged and absolute rest in the recumbent posture is of little avail, while their proneness to return soon after the erect posture is assumed is a matter of everyday experience; but by the proper and systematic use of the strong elastic bandage, a cure may be effected in a comparatively short space of time, and that, too, while the patient pursues his usual avocation, "without," as Dr. Martin says, "the necessity of an hour's rest or change of posture," and re-ulceration may be surely prevented by continuing the use of the bandage, by which the occurrence of venous stasis and its accompanying evils may be averted.

During the past twenty-five years Dr. Martin has treated at least six or seven hundred cases of ulcerated legs in this manner, "and all, without exception, have been perfectly and absolutely cured." But lest this statement may lead any one to suppose that the bandage alone may be relied on for the cure of all ulcers, even those arising from constitutional dyscrasia, it

must be understood that Dr. Martin distinctly excludes syphilitic and scrofulous sores, saying that, although often proving a very useful adjunct to treatment, the bandage alone is not sufficient, appropriate internal medication being required before resolution can be effected.

The treatment of ulcers by Dr. Martin's method, although only recently introduced into this country, has attracted considerable attention, and its usefulness has already been attested by two very competent and trustworthy observers, Mr. Jonathan Hutchinson, of the London Hospital, and Mr. Callander, of St. Bartholomew's, and my own comparatively slight experience of it is of such a character as to convince me of its utility, and to encourage me in continuing it.

As an illustration of the good effects of the mode of treatment under notice, I bring before you a patient who became an inmate of one of my female wards three weeks ago. When admitted she was suffering from a varicose ulcer of the leg. The ulcer was about two and a half inches in length, and one and a quarter in breadth; its surface was covered by the usual unhealthy-looking grayish granulations, and it was surrounded by a wide halo of inflamed skin. On 16th October a strong elastic bandage was applied. Two days afterwards the ulcer was found to be diminished in size, a distinct rim of new epidermis having invaded the ulcerated surface. In twelve days' time it was half healed, and now—exactly three weeks since treatment was commenced—it is all but whole. The little spot that still remains uncovered by epithelium is as healthy-looking as could be desired, and the inflammatory redness has completely disappeared. This patient had previously suffered from varicose ulcer, and on that occasion she was an inmate of an hospital for two months, and was confined to bed for nearly the whole of that period. The case is therefore a striking and convincing one, and so satisfied is she with the mode of treatment adopted on the present occasion, and with the comfort afforded by the bandage, that she has purchased one for future use. The directions given as to the manner of applying the bandage are as follows:—The patient should put it on while in bed before the veins of the leg become distended with blood, which they do immediately after the erect posture is assumed. The bandage should be applied by winding one turn just above the malleoli, then one round the instep and sole, then up the leg, spirally, round and round, to the knee, each turn overlapping that below it from one-half to three-quarters of an inch. If there is any redundant bandage it can be wound round the leg below the knee, the tapes carried in different directions, and tied. It should be applied with just sufficient snugness not to slip down. The moment after the foot is put to the ground,

the limb is so increased in bulk by the increase of blood in its veins that the bandage becomes of precisely the proper degree of tightness, and, no matter how active the exercise or labour of the patient, it will remain in position all day. When the patient undresses at night the bandage is to be removed, and the limb wiped dry; a piece of soft old linen moistened with olive-oil, or some equally simple dressing, laid on the ulcer, and retained in place by a few turns of an ordinary roller. The bandage should be sponged with water (cold will do, but warm is better), and hung over a line to dry, in readiness for the morning, or it can be wiped dry at once, and rolled up, with the tapes in the centre. In the morning the leg should be washed, and all traces of oil or cerate should be carefully wiped away, as contact with any fatty matter would tend to injure the rubber of which the bandage is made. The use of stimulant or antiseptic applications is uncalled for; therefore the treatment is of so simple a character that it can be easily carried out by even the most ignorant patient without the aid of medical supervision. When giving instructions regarding the proper mode of applying the bandage, the patient should be told that during the first week or two an eruption will probably appear about the parts, but that no treatment is required, as it will disappear spontaneously. The curative effects of the bandage in cases of varicose ulcer may be ascribed to the support afforded to the veins, the destruction of their valves, and the loss of contractile power of their coats being compensated for by the regular elastic support which it affords,—to the absorption of the products of inflammation being induced by the constant pressure it exerts, while granulation growth is favoured by the moist, humid atmosphere to which the ulcer is constantly exposed, the granulations, and tender young epithelium, being at the same time protected from external injury by the soft and regular surface of the rubber.

Dr. Martin speaks favourably of the use of the bandage in the treatment of varices of the legs unaccompanied by ulcers, and advances the opinion that it will not only prove palliative in such cases, but that a cure may be achieved; on this latter point, however, the evidence he adduces is very meagre. Of its palliative influence, and of its great superiority over the elastic stockings in common use, I have myself had striking and convincing proofs. Regarding its curative effects, I am as yet unable to supply any data, but keeping the pathology of the affection in view, I feel somewhat inclined to doubt the possibility of a real and permanent cure ever being attained without having recourse to operative procedures.

Before leaving this part of the subject, I would point out that the elastic web bandage is not only equally suitable for the

treatment of varices unaccompanied by ulceration, but that it seems to me to be preferable to Martin's bandage, for, on account of the openness of its texture, free circulation of air about the parts is allowed, and thus the occurrence of any irritation arising from confined perspiration is avoided, while the appearance of the web bandage renders its use more acceptable to fastidious patients than that of the somewhat clumsy-looking india-rubber one is likely to do. For those to whom expense is an object, Martin's bandage should be ordered, for although its original cost is somewhat greater, ultimately it is really cheaper, as it lasts for an indefinite period, while the web bandage gradually loses its elasticity, becoming less and less elastic after each occasion on which it is washed. An Esmarch's bandage would undoubtedly be more elegant than either of those mentioned, but it would be found by far too expensive for general use.

The second class of diseases in which elastic compression will be found of value is a very important one—viz., affections of joints, including, among others, tumour albus, or gelatinous transmutation of synovial membrane, effusion into joints, and sprains. In the first mentioned—white swelling—I have found elastic compression invaluable, and I firmly believe that in the web or rubber bandage we possess a means of treatment capable of effecting more good than any of the modes yet devised, and that results may now be obtained which, before the introduction of elastic bandages, would have been deemed impossible. In the commencement of this paper I mentioned that, for some time previous to the publication of Dr. Martin's paper, I had been employing elastic bandages in the treatment of disease, and it was in cases of this kind that I first used them, the results obtained being of a most satisfactory and unlooked for character.

It is unnecessary to occupy time with details, but I may mention that two of the cases were recently in Martha's ward, and that their steady progress was watched with much interest, and evident satisfaction, by a large number of students, and by several practitioners, who from time to time accompanied me during my visits. Ultimately I had the pleasure of directing attention to the fact, that, by the systematic use of elastic pressure, the abnormal appearances had been entirely dissipated, and that the previously affected joints were now as sound as their fellows. Both cases were well-marked examples of pulpy degeneration affecting the knee-joint; in one the disease had existed for years, during which the child had been subjected to various modes of treatment, but without any good resulting, and so obstinate had the disease proved that operative treatment, at no distant date, had been held out to the parents as

the only means by which there was any hope of the child being restored to health and ultimate usefulness. The time occupied by treatment was of considerable duration, still the results amply repaid the exercise of patience on the part of both patient and surgeon. The bandage used was the elastic web one—and here it seems to me to be more suitable than the other, as it can be applied with greater nicety to the unequal surface which it has to embrace; and as it should be worn constantly both during day and night, the free circulation of air allowed by the open texture of the bandage is a decided and evident advantage.

Dr. Martin does not seem to have made use of his bandage in the disease just mentioned, but he has employed it with manifest success in cases of effusion into joints, where I have myself found elastic compression most useful. In chronic cases he advises aspiration, followed by the application of his bandage; in recent cases the bandage alone suffices. Regarding the combined use of aspiration and compression, he says, “I have never known a case in which this was done where effusion returned;” and, conversely “I have never known a case where such a joint was aspirated, and no such support afterwards applied, in which effusion did not return, whether the synovitis was the result of injury or of disease.”

After aspiration the bandage should be worn *continuously*, both day and night, for at least six weeks, but rest need only be prescribed for the first two or three days, after which the patient may resume his avocations with perfect safety, re-effusion being prevented by the firm pressure exerted by the elastic covering in which the joint is encased. If the bandage be removed for but a single day—especially during the early part of the treatment—the synovial sac may again become distended with fluid, therefore strict injunctions for its constant use should be given, and the patient should be warned of the probable undesirable effects which may follow the disuse of the appliance for even so short a period as twenty-four hours.

In sprains the use of the bandage will at once recommend itself to all, for the employment of elastic compression and support in cases of the kind is as sound in theory as experience has proved it to be in practice. In what is known in common parlance as a sprain there is generally rupture in a greater or lesser degree of some of the ligaments, attended by extravasation of blood, and followed by exudation of inflammatory products; and no better substitute for the torn ligaments could be found than that supplied by an elastic bandage, and no more potent method could be devised for the prevention of undue effusion, or for setting up its absorption, than the gentle, yet continuous, compression which it affords.

The so-called antiphlogistic treatment of sprains by leeches, fomentations, and lotions, has for the most part been replaced by the more rational one of pressure and immobilisation from the first, while the correctness of Velpeau's statement that "compression is the sovereign resolvent in contusions with infiltration and swelling," is universally allowed. Many excellent methods of treating sprains on this principle have been suggested, such as the strapping system advocated by Baynton, the immovable apparatus of Seutin, and the cotton-wool dressing, the paste-board splints, and accurate bandaging of Sampson Gamgee; but the elastic bandage seems to me to be possessed of advantages peculiar to itself—advantages of no small moment to both patient and surgeon—for suffering is lessened, a more certain and speedy cure is effected, and the means by which such desirable results may be obtained are of the simplest possible description. The firm support afforded by several folds of the strong elastic bandage is as efficient in maintaining physiological rest of the wounded part as splints or fixed bandages, while the *bête noire* of this latter mode, viz., the remodelling of splints, and the removal and replacement of stiff bandages, necessitated by the gradual shrinkage of the limb as absorption goes on, is avoided, for the elastic bandage contracts as swelling diminishes, and even if it has to be reapplied it is only the work of a few minutes, and thus the time and trouble involved are much lessened.

Moreover, absolute fixation of joints by stiff bandages is not unfrequently attended by most undesirable sequelæ, for, as Martin remarks, it is "very apt to lead to a very decided stiffening, sometimes to a degree, permanently impairing the usefulness of the articulation, and nearly approaching ankylosis;" but when the elastic bandage is applied in such a manner as to act as a temporary substitute for the torn ligaments, it affords a firm, strong, constantly resisting force, by which distortion of the part is prevented; while it still permits the natural motion of the part to an extent sufficient to render the occurrence of subsequent stiffness of the articulation extremely improbable.

When using the bandage in the affection now under notice, "the one thing to be borne in mind," says Martin, "is that the bandage is a temporary substitute for the injured ligament or ligaments, to take the place of them, and so permit them to have that undisturbed rest which is absolutely essential to their perfect repair. The application of the bandage must, therefore, be so made as to support the joint in such a way as to prevent motion to an extent or in a direction which would apply extension to the injured ligaments."

The exact method in which the bandage should be applied,

therefore, depends upon the character of the injury, and upon the nature and degree of the displacement, should any be present, thus it is impossible to give any specific directions for its use; but, if the practitioner has any just ideas of the true indications of treatment, he will experience but little difficulty in fulfilling them.

In many fractures of the extremities there is a wide field of usefulness for elastic compression, for, as in sprains, undue swelling is prevented, absorption of bloody and other extravasations is accelerated, and the splints being held in position by a constantly contracting elastic bandage continue to act as efficient supports to the fractured bones for a much longer period than when they are retained by an ordinary roller. On account of the manipulation of the injured part being thus reduced to a minimum, union proceeds more speedily, and a saving of pain and anxiety to the patient, and of trouble to the surgeon, are effected. In fractures of the ribs I have not yet tried the elastic bandage, but I expect that it will prove most useful in such cases, as I have found that the web bandage when applied to the thorax kept its position perfectly notwithstanding the active exercise of those who were the subjects of treatment. The superiority of a tight-fitting elastic covering (such as that afforded by a carefully-applied web bandage) to the strips of plaster or binders in common use in cases of fractured ribs, seems so manifest that it is unnecessary to enter further on the subject.

Martin's experience of his bandage in fractures has apparently been somewhat limited, as he only speaks of its use in a case of "green-stick" fracture, and in three cases of faulty union of recent fractures where he succeeded in overcoming the resulting deformities by means of his appliance; and although the strong elastic bandage would doubtless prove efficient enough, it was to the use of the web bandage that I referred when just now speaking of the value of elastic compression and support in the treatment of fractures.

In chronic abscesses I have found aspiration followed by elastic compression most useful. In some cases obliteration of the abscess cavity has followed a single introduction of the needle, and of this I had recently an excellent example in Anna's ward. The abscess was one of considerable size and standing in the upper part of the left thigh of a young female. It was aspirated, and a considerable quantity of strumous pus was allowed vent; the part was then firmly bandaged with an elastic roller; no re-accumulation of fluid took place; and when the patient was discharged, no trace of the swelling was discernible.

In sinuses so situated as to permit the use of the bandage, it

proves a valuable adjunct to the treatment introduced by Volkmann, for if a bandage be applied after the sinus has been thoroughly cleared of the unhealthy granulation tissue by which it is lined by means of the sharp spoon, its closure will be greatly hastened by the exercise of external elastic compression.

In enlarged bursæ, treatment similar to that advocated in chronic synovitis will be found safe, efficient, and satisfactory.

The remaining affections in which Martin has employed his bandage with success—viz., œdema of the limbs, rheumatic and neuralgic affections of joints, erysipelas, and some cutaneous diseases, and as a temporary dressing after dislocation—call for no special notice, although his observations are both interesting and instructive. And now, as I fear that my remarks have already extended to an undue length, I must bring this paper to a conclusion. In doing so I would say that I have endeavoured to lay before you as concisely as possible some of the advantages attending the use of elastic compression, as seen by Dr. Martin and myself; and I trust that what I have said may induce some of the members of the Branch to give a fair and impartial trial to a mode of treatment which has much to recommend it, and which, I feel assured, will occupy at no distant date a sure and prominent place in surgical therapeutics. —*Edinburgh Medical Journal*, March 1872, p. 782.

54.—THE INDIA-RUBBER BANDAGE FOR ULCERS AND OTHER DISEASES OF THE LEGS.

By Dr. HENRY A. MARTIN, Boston, U.S.A.

I am extremely anxious that my method of treating, not only old ulcers, but all ulcers, and particularly varicose ulcers, of the leg, and not only these, but many other diseased and impaired conditions, especially of the lower extremities, by the use of strong and perfectly elastic bandages of “pure rubber,” should be fully and *accurately* presented to the notice of the profession of Europe. I have practised this method for very nearly twenty-five years; at first for ulcers and eczema of the legs only, but subsequently its admirable applicability to other cases has been ascertained. During all this long series of years, I have thus treated a very large aggregate of cases; have thoroughly learned the really marvellous rapidity, thoroughness, and solidity with which cures are thus accomplished of cases and classes of cases which largely yielded under other methods more or less complete *opprobria* of surgery. All that I have written on this subject, however exaggerated and over-confident it may appear, is the result of almost innumerable observations. Not one word has been published which has not

been most fully verified in my own experience, or which cannot be easily and certainly confirmed by the observation of those who choose to take the perhaps unusual but certainly judicious course of fully and fairly testing a perfectly safe and simple method previously to uttering critical judgment upon it. Although so long practising the method of treatment by the "pure rubber" bandage, and freely communicating my experience in conversation with my friends, I never made a wider announcement till at the meeting of the American Medical Association at Chicago in June 1877. I then made somewhat extended extemporaneous remarks on the subject, which by the vote of my hearers, was subsequently included in a paper published in the volume of Transactions of the Association for 1877. In accordance with very numerous and urgent requests, I also published a paper in the Chicago Medical Journal and Examiner for November 1877. These two publications made the method very generally known throughout this country. I learned that an innovation in practice which claimed so much, announced by one so far from eminent as myself, might meet with but scant notice; but in this I was most agreeably disappointed. I have, in person and by letter, received the thanks of hundreds of American practitioners, and their assurance that the results in their hands of the practice I advocated had fully confirmed all and much more than all that I had asserted.

As the publications to which I have alluded are probably not readily or at all accessible in England, I think it proper to briefly state, for the information of English surgeons, the manner in which I apply the pure India-rubber bandage for the cure of ulcers and eczema of the legs. Before doing this, I would simply enumerate the other cases in which I have in actual practice found the bandage to be of unequalled value.

1. Certain diseases and injuries, and results of disease and injury of joints, particularly of the knee and ankle. Among these, I would especially mention synovitis, both acute and chronic. In acute cases, even where effusion is considerable, by the bandage alone; in chronic cases, even with slight effusion, after aspiration; the bandage to be worn continuously day and night for a sufficient length of time. Also in every variety, recent or of long standing, of "sprain," and the very numerous cases of weakness, relaxation, and pain on exertion of the knee and ankle; and also in much less frequent instances of the elbow, wrist, and great toe-joints.

2. In diseases of the bursæ mucosæ, particularly in the disease called "housemaid's knee," after perfect aspiration. I have treated seven of these cases, and one of great effusion and consequent enlargement of the bursa patellæ, with perfect and immediate success.

3. As a very efficient palliative in œdema and anasarca of the lower extremities, and of that extreme and painful œdema of the arm which is one of the many miserable complications of cancer of the breast. I have treated two of these latter cases with the effect of reducing the arm to nearly its normal size in forty-eight hours, and with great relief to the patient.

4. An inestimable palliative, and, even when worn continuously for long periods, absolutely curative of varicose disease of the leg. I have had many patients, and have heard of many more, who have substituted the bandage for the very expensive elastic stocking, and thus obtained, at an entirely insignificant expense, palliation incomparably superior to what they had previously experienced, and with a good prospect, if the use of the bandage should be persisted in for a year or more, of great permanent and even radical relief and cure.

5. In the often extremely distressing œdematous and varicose condition of the lower extremity in pregnant women. Besides the great relief, while in the erect position, thus afforded during the continuance of pregnancy, the permanent varicose disease which follows repeated pregnancies, or even a single pregnancy, is, by the use of the bandage, most surely averted. I have had many patients who have used the same bandage in several successive pregnancies.

6. In cases of phlebitis and phlegmasia dolens, the effect of the bandage is in every way admirable, as will be readily believed by every one who has observed the effect of a single day's application; warmth, pressure, moisture, and firm and constant, but gentle and equable, support are what the bandage affords, and are precisely what are most desirable in these cases.

7. In cases of cellulitis of the leg and thigh. If applied in the very earliest stages, I know no means so likely to prevent suppuration; but if, as is generally the case, applied too late to prevent abscess, I am aware of nothing, for obvious reasons, so likely to promote healthy and prevent diffuse suppuration. After the opening of an abscess in the thigh, for instance, the bandage, properly applied, is of quite unequalled value in promoting the constant evacuation of the cavity, securing its perfect, rapid, and solid closure, and preventing the formation of sinuses and fistulæ.

8. The bandage has been found very useful in a few cases of "greenstick" fracture and vicious union of recent fracture. Surrounding a proper splint, it has afforded a constant and considerable but gentle pressure, which has served to gradually but perfectly correct irregularities of form, and, in the first named cases, has accomplished that perfect reduction which is not always obtainable by the application of great force.

9. Besides its unequalled value in eczema of the legs, the

bandage has been found extremely useful in many diseased states of the skin, and is very confidently recommended in any case, however named, which is caused and maintained by irregular circulation and consequent defective or redundant nutrition, by deficient moisture or pliability, due to torpor or obstruction of the cutaneous glands and follicles, or redundant proliferation of epithelial cells. This list, long and pregnant as it is, does not include all those cases in which other physicians as well as myself have found the pure rubber bandage very useful; but I will not continue it further than to state that Dr. Boutecou, of New York, one of our most justly eminent surgeons, has communicated to the journals his very satisfactory results in treating by these bandages, not only phlegmasic enlargements and abscesses of the female breast, but in arresting the growth, and even gradually obliterating adenoid and fatty tumours of the mammæ.

In the treatment of the various cases I have enumerated, bandages of varying length, width, and thickness are employed. In a few cases it is desirable to apply a bandage from the foot to the groin. The length of such a bandage should be about seven yards; its width three or three inches and a half; its thickness, if the case be one of phlebitis or varicose disease, No. 21 of Stubbs's wire gauge; No. 19, if more support be needed, as in a case where, with other disease, there should be weakness or disease of the joints. If the case be one of eczema or other cutaneous affection, only a much thinner bandage will meet all required indications which, in such cases, are not pressure and mechanical support, but exclusion of air and profuse diaphoresis and its results. In disease or injury of the ankle, I use a bandage five feet long, three or three and a half inches wide, and No. 19 (Stubbs's gauge) in thickness. For the knee, one of seven feet and a half, and the same width and thickness as for the ankle. In many cases of knee-trouble, I prefer a bandage fifteen feet long, three inches and a half wide, and No. 19 thick, to be wound from the foot upward, including the knee; for, if applied to the knee alone and with sufficient closeness, an undesirable amount of oedema of the leg would be induced. All the bandages have a strip of strong linen inserted in one end, to which is strongly sewed a stout double tape eighteen inches long. The material is what is here technically called "pure rubber," *i.e.*, the best Para india-rubber "cured" with the minimum of sulphur necessary to produce with heat that wonderful effect of vulcanisation which, if properly accomplished, renders the gum practically indestructible and of perfectly and permanently resilient elasticity. The bandages which formerly I was obliged to have manufactured for my own use are now produced by the manufacturers of india-rubber throughout

this country, and can be obtained from all the dealers. If there should arise any demand for them in England, they can, of course, be produced there as well or better, and certainly at a lower price, than in America.

For the treatment of ulcers of the leg, its most important and frequent application, the bandage should be three yards and a half (ten feet and a half), not *twelve* yards, as Mr. Hutchinson is made to say in the report, three inches wide, and of the thickness of No. 21 (Stubbs's gauge). It should be rolled up with the tapes inside. It is to be applied directly to the skin and surface of the ulceration without any lotion, salve, or other dressing whatsoever. The statement attributed to Mr. Hutchinson, that a piece of lint may be placed over the ulcer, has no authority from me. An essential point in the method is that *nothing whatever* shall be applied between the bandage and the ulcer. In a very few cases, a hyperæsthetic patient has been, or thought he (or she) was unable at first or at all to endure the rubber next the skin, and, in these cases (not one in a hundred), I have directed that a linen, cotton, or thin flannel "roller" should be applied to prevent the immediate contact of the rubber. I have done this rather than renounce the great advantages of the india-rubber bandage entirely; but it always delays the cure of the ulcer. In cases of uncomplicated varicose disease, or in many joint cases in which the important use of my bandage is the support and security it affords, there is no objection to the application of an ordinary bandage under the rubber; but, in cases of ulcer, eczema, and other cutaneous diseases, and many where the joints are affected, it is necessary, for the full attainment of the benefits of the treatment, that the bandage should be in immediate and close contact with the skin. The bandage should be applied in the morning, before the patient has for any length of time been on the feet. The best way is to apply it in bed before rising. One turn is to be taken just above the malleoli; then obliquely round the foot and again up and round the ankle, just below and overlapping the first turn; and so spirally up the leg, round and round, without reduplication, to the patella, just below which any superfluous bandage may be wound, the tapes separated, carried in opposite directions, and tied. Each turn of the bandage should overlap the preceding from one half to three-quarters of an inch. If it be applied in bed, it should be wound with just enough snugness not to slip down. When the patient assumes the erect position, the leg at once enlarges, from the accumulation of blood in its veins, and the bandage becomes of precisely the requisite closeness of application, and may be worn all day without readjustment, no matter how active the exercise or labour of the wearer.

If the bandage be applied *after* the patient has been on his feet, it should be wound with sufficient closeness to put the india-rubber at each turn *slightly* on the stretch. If it be properly applied, there should be no unpleasant sense of tightness, but merely of comfortable and easy support. The patient should wear the bandage all day; at bedtime, it should be removed. The limb as well as the bandage will be found bathed in moisture. The leg should be wiped dry; a piece or pieces of oiled linen rag should be applied to the ulcerated surface, and retained by a few turns of an ordinary roller. The bandage should be rinsed with clean water and hung up to dry, to be ready for reapplication the next morning. In some cases, where the secretions are foetid or the patient filthy, it is well to rinse the bandage in carbolised water. In such cases, I advise that the leg should be washed with tar or carbolised soap and water. This antiseptic measure is proper and advisable, but does not seem to be essential to the treatment; for I have seen many cases where there was an utter neglect of everything like cleanliness, and in these cases results were as rapidly and completely successful as in those in which the greatest regard had been paid to the proprieties. In the morning, when the bandage is to be reapplied, all oily or greasy matter should be wiped from the stem with a dry rag. This is the entire treatment: india-rubber bandages worn while in the upright position; a simply defensive dressing while lying down. In a few days after the bandage is first applied, papules, often in considerable number, appear; these, if the treatment be continued, very rapidly suppurate, discharge, and disappear, not to return. Each of these pimples indicates an obstructed follicle. They sometimes look quite angry, and even resemble minute furuncles. Under the bandage, they run a very rapid course, leaving the skin in a perfectly healthy and unobstructed state.

After the treatment has continued for a week or ten days, the skin becomes of a rosy colour, and is sensitive to a degree which sometimes alarms the patient. This is due to the perfect maceration and removal of the outer and effete layers of epidermis, and this most perfect removal of all obstruction to the discharge of the normal functions of the skin is, without doubt, the principal reason for the very remarkable effect of this treatment in certain cutaneous diseases, particularly eczema. It is well known that the skin of ulcerated legs is often, indeed generally, in a very unhealthy condition, dry, scurfy, covered with pimples, and, not unfrequently, with an eczematous eruption. I have never known such an instance in which, by the time the ulcer has been thoroughly cicatrised, the skin has failed to be restored to a condition of perfect health. In old cases of ulcer in elderly, poorly-nourished people, this change is

very marked indeed. The dry, almost parchment-like surface, shedding epidermal scales at a touch, becomes perfectly soft, of satin-like smoothness, and totally free from lichenous, papulous, and eczematous eruption.

The results of this treatment are most marked, in that very large class of ulcers of the leg complicated with varicose disease of the superficial veins. Progress towards cicatrisation can be noted at the end of the second, or even of the first day, and from day to day. The cicatrisation is firm, smooth, and enduring, far beyond what I have usually observed as the result of other treatment. It is a noticeable fact that, under the bandage, the scars of old ulceration, unless exceptionally sound and smooth, break down and are rapidly replaced by new and better cicatricial tissue. In old, indolent, hard-edged ulcer, the effect of the bandage is not so rapidly curative, but far more so than by any other method that I have ever observed. I have treated these cases by the bandage alone, but there is no doubt that the results of treatment may be much sooner attained by the destruction of the indurated, almost cartilaginous, margin, by the knife or potential cautery. I have alluded to the treatment of eczema by this method. The details of treatment are almost exactly the same as for the ulcers which are so frequently complicated with eczema. The only difference is, that in the latter disease it is well, with a view to a rather more rapid cure, to wear the bandage night and day, removing it morning and evening for the purpose of renewing the bandage and wiping or washing the limb. In the treatment of ulcers, the bandage need be worn during the day only. Although I have had a few patients who preferred the bandage at night to a merely defensive dressing, the great majority prefer the latter, and the continual use of the bandage offers no real advantage in any case of ulcer of the leg, unless, perhaps, those of a very indolent character, with very hard thick edges, in which the pressure of the bandage alone is relied upon to induce the absorption of the latter.

The great advantages of the method are the following. The patient is not for a moment obliged to discontinue his occupation, however laborious that may be. So far from this, indeed, that the application of the bandage enables many, principally of those suffering from varicose disease, whether complicated with ulcer or not, to labour with ease and freedom from pain, who without it could only labour painfully or not at all. The bandage accomplishes, even more perfectly, the one important end gained by observance of the recumbent position; viz., relief to the embarrassed circulations and consequent impaired nutrition of the skin. This is, however, by no means the only advantage that the bandage

offers, and patients working every day and all day progress more rapidly and to a more perfect and solid recovery than if strictly confined to the recumbent position without the elastic bandage. When the circumstances of the vast majority of the sufferers from aggravated varicose disease and ulceration of the legs, and their almost utter inability to cease from labour for any great length of time, are taken into consideration, the great value of a method of treatment which does not necessitate a cessation of that constant labour, which is the hard condition of mere existence, must be at once admitted. 2. The labour of the surgeon, in treating a very numerous class of patients demanding relief but generally able to afford but very scant *honoraria*, if any, is by this method reduced to a minimum. As I have said, a single interview with the patient, a demonstrative application of the bandage, and some brief and simple directions, are all that each case requires.—*British Medical Journal*, Oct. 26, 1878, p. 625.

55.—THE SOLID RUBBER BANDAGE.

By Dr. HENRY A. MARTIN, Boston, U.S.A.

I wish to say a word or two in regard to the sort of bandage I recommend. In all the papers I have written on the subject, I have directed the use of bandages of a certain thickness, as being, on the whole, the best.* Bandages somewhat thinner would undoubtedly fulfil almost all indications in a large proportion of cases; and *very much thinner* bandages would meet the requirements of certain cases, as of some cutaneous affections, particularly eczema, if uncomplicated with a varicose condition—*every* requirement, indeed, excepting one, viz., durability. The thinner the bandage, the lower the price at which it can be obtained, even if the materials be the best; but, taking into consideration the fact that very thin rubber bandages are very apt to be torn, to be stretched out of shape, and to be acted on by moisture in a way that renders them

* The thickness recommended is 19 (Stubbs's wire gauge) for bandages for the knee and ankle; and in some cases, as of œdema, in which firm and considerable pressure is desirable, No. 21 in the very large majority of cases in which the rubber bandages are recommended. Of course, bandages a little thinner would often answer in many cases, but no better than those I advise; and it is very desirable to simplify the matter, and not to have a useless and confusing multiplicity of sizes and thicknesses. The width recommended is three inches and three and a half inches; the latter width for bandages for the knee and ankle, and where it is necessary to apply a bandage from the foot to the groin. The width of three inches and a length of ten feet and a half are proper for bandages for ulcer, eczema, &c., of leg below the knee. Smaller bandages six feet in length, two and a quarter inches wide, and twenty-one inches (Stubbs's gauge) thick, are made for children's legs, and for the wrist, &c., of adults, but are rarely required. The bandage of ten and a half feet long, three inches wide, and No. 21 thick, meets the requirements of ninety per cent. of all the cases to which the pure rubber bandage treatment is peculiarly applicable.

irregular and uneven, and on all these accounts that they will often require to be renewed in the treatment of even a single case, while one of the thicker bandages, of proper quality, is not only sufficient for the treatment of a single case through its whole course, but for the cure of many successive cases, I have no hesitation at all in repeating my recommendation of bandages of the thickness indicated in my paper in the Journal of October 26th, as on all accounts the best, and, although they cost somewhat more, in the end the most economical. As it is my hope that this method may be largely introduced into hospital and infirmary practice, and so become a benefit to the vast masses of the suffering poor, I am anxious to be understood on this point clearly. The use of very thin rubber bandages was never recommended by me. A very eminent practitioner of New York, who early after its publication adopted my practice, is their suggestor, on the ground, I believe, that they are cheaper. Bandages of the thickness I recommend certainly fulfil all indications as well as if of any lesser thickness, and some very important indications in large classes of cases very much better. The only objection to them of any force whatever is their slightly greater first cost. If gentlemen, however, see fit to use the thin bandages, I would suggest one thing that they should *not* do, viz., apply them *double*. If this be done, evenly diffused constantly equable pressure in all parts of the limb cannot be obtained, or at any rate so perfectly obtained, as where the bandage is applied of *single* thickness. This may be proved by actual and easy experiment.

Another point to which I wish to allude is that, in applying a pure rubber bandage to the knee-joint, its edges, particularly the lower edge, are apt to chafe and even to cut the skin. I have often met this by applying a strip of sticking plaster to the skin at the point with which the edge of the bandage comes into contact, and this answers pretty well; but I now procure to be made for me knee-caps of pure rubber of the same thickness as used for the bandages. These knee-caps are simply tubes of pure rubber of different diameter and of proper thickness—just, in fact, as if from one leg of a “skin-fitting” pair of rubber tights one should cut out a section including the knee and three or four inches above and below the joint. For about three-quarters of an inch from the top and bottom, these tubes are of a slightly greater diameter, so as to “feare out” like the lips of a trumpet. In practice, I find these caps to fulfil all the purposes of the bandage, and to be quite free from the annoyance I have mentioned.

All that I have said of the durability of the rubber bandage, and its consequent economy as an appliance of hospital treatment, supposes the bandage to be of proper thickness, well

made, evenly cut, and of the very best material, viz., Para india-rubber, properly "cured." I mention this, as instrument-makers here, contending to produce bandages at the lowest price, have made an article which *looked* very well, but went to pieces in a week or two of actual use. It may not be generally known that there are many different varieties of rubber, very widely differing in quality and price. Another fact is, that many other gums, of next to no value, may be combined with rubber in almost any proportion. As a result, "India-rubber" bandages may be produced at almost any price, and, as I have stated in one of my former papers, of an *appearance* even more attractive than those made in the most honest way and of the very best materials. The only true test is use. If a "pure rubber" bandage stretch out of shape, become brittle or "gummy," and so worthless, in a short time, it is not what it ought to be. A pure rubber bandage of proper thickness, properly cut, and made of pure well "cured" Para rubber, is almost indestructible, and should at the end of two years' constant and careful use be as good and useful as when first made. I hope, as it seems probable that my bandages may come to be in considerable demand in England, that instrument-makers, while they produce them at the lowest paying price, will not do so at any sacrifice of labour in making or of excellence of material.—*Brit. Med. Jour.*, Dec. 14, 1878, p. 874.

56.—THE OLD PRINCIPLE OF THE NEW ELASTIC BANDAGE.

By SAMPSON GAMGEE, Esq., F.R.S.E., Surgeon to the Queen's Hospital, Birmingham.

No surgeon can have failed to read with the deepest interest in these columns (October 26th) Dr. H. A. Martin's memoir "On the India-rubber Bandage for Ulcers and other Diseases of the Legs." Our Boston brother may justly claim to have introduced and established the use of a new surgical appliance of unquestionable value; but its principle of action is old. Dr. Martin very energetically rebuts a suggestion which was made at the recent Bath meeting by Mr. Solomon that "the india-rubber bandage is only a very slight modification of the domett bandage" used by "colliery surgeons in the treatment of chronic ulcers." "There is no analogy," Dr. Martin assures us, "between india-rubber and cotton, nor is there any real resemblance between the effects of bandages of these two substances in the treatment of ulcers of the leg." With much deference, I submit that the resemblance amounts to very close analogy. Dr. Martin's india-rubber bandages, with which Messrs. Salt and Son have supplied me, are of beautiful

structure, and most readily adapt themselves to the limb so as to exercise gentle uniform compression. In principle, the condition is practically identical of two limbs, of which one is bandaged with Martin's india-rubber, and another nicely strapped with plaster and rolled with Churton's bandage. Both are excellent methods of exercising that uniform compression of which the therapeutic value was first adequately taught, towards the close of the last century, by Thomas Baynton, surgeon, of Bristol. His unpretending little volume is one of the gems of surgical literature—simple, truthful, and convincing. John Scott's more ambitious, yet excellent, though almost forgotten, work inculcated the same lessons, which, in our generation, have had such apostles as Seutin and Velpeau.

Far from me to wish to detract from the legitimate and intrinsic merit of Dr. Martin's contribution to our practice. I rather hope to furnish additional reasons for its adoption, by showing that its value depends on old and proven, though insufficiently recognised, scientific principles.—*British Medical Journal*, Nov. 23, 1878, p. 762.

57.—ON WOUND TREATMENT, BY DRY AND INFREQUENT DRESSING, REST, AND PRESSURE.

By SAMPSON GAMGEE, Esq., F.R.S.E., Surgeon to the Queen's Hospital, Birmingham.

In wound treatment *pressure* is a most powerful and beneficial agency. It favours union by maintaining accurate coaptation, and prevents extravasation of blood and its products. When such extravasation has occurred, pressure is the quickest, the least painful, and the most successful agency in promoting its absorption.

A patient, H. B., a butcher, aged twenty-three, was admitted to Ward 1 on Jan. 3rd, at 11.30 p.m., having just previously slipped in the slaughter-house. There was a fracture about two inches from the tip of the right external malleolus, the internal lateral ligament was torn, and the foot freely movable laterally. The ankle was perceptibly swollen. Hot fomentations were ordered to be constantly applied.

The foot and lower two-thirds of the leg having been enveloped in a layer of fine cotton-wool, the ankle was immobilised with strips of moistened pasteboard, compressing bandage, and heavy lateral sand-bags.

The pain rapidly subsided. At the expiration of eighteen hours the man could lift the foot from the bed in comfort and without help. As slackness of the bandage indicated considerable subsidence of the swelling, I applied another roller, with firmer pressure, leaving the patient in perfect comfort.

8th.—5 p.m. (just four days after immobilisation and compression): The patient continues quite free from pain. On removing the dressing, and measuring the limb at the inkmarks left on Jan. 4th, the dimensions are:—Centre of heel, $13\frac{1}{2}$ in. (decrease an inch and a half); five inches from sole of foot, $9\frac{1}{8}$ in. (decrease five-eighths); nine inches from sole of foot, $10\frac{1}{2}$ in. (decrease half an inch). Ecchymosis extends from the roots of the toes to the knee. Skin cool and shrivelled.

11th.—1.30 p.m. (eighth day after admission): Continues quite easy; apparatus loose, taken down, and splints remoistened and bandaged with steady firmness. Ecchymosis of deeper hue and more extensive. The man was discharged, with instructions to attend as out-patient.

Another patient, J. M., a builder's foreman, came in to consult me from the country the 20th of December ult. The day previously, while working on a scaffold, he was knocked down by a falling mass of brickwork, severely bruising his right ankle. The boot was removed with difficulty, and the joint was at once found to be a great deal swollen and excessively painful on the slightest movement. He was removed home, and hot fomentations applied all through the night. The pain became more intense, and the swelling increased; sleep was impossible. When I saw the case, about twenty hours after the accident, the swelling of the lower part of the leg was so great that it was impossible to say if there were any fracture. Measurement round the point of the heel and ankle proved the injured side to be two inches more in circumference than the sound one. I covered the foot and lower half of the leg with cotton-wool, surrounded the ankle with strips of moistened pasteboard, applied a gently and evenly compressing bandage, and, with two outside strips of dry pasteboard, the man was removed, in comparative comfort, into the vehicle which had brought him in great pain. On returning home, he states that he was soon easy and fell asleep. I did not touch the limb until the 24th of December (fourth day), when the looseness of the bandages denoted the great subsidence of swelling. On removing the apparatus, deeply mottled bruising, quite purple at the back, extended from the roots of the toes up nearly to the knee. The outline of the ankle was now quite distinct, and there was scarcely any pain. I again covered with cotton-wool, and firmly rebandaged the splints which had just previously been remoistened, so as to ensure their accurate fit under the moulding pressure. At the expiration of a week the man was able to inspect building operations, and he has suffered nothing since, though it will, of course, be some time before he can regain full use of the joint.

A ready method for immobilising joints in similar cases is to break off strips of thin rough unglazed millboard, measuring about twelve inches in length and two in breadth. Placing them on a dish or metal tray, they very rapidly become quite soft on a jug of hot water being poured over them. The uneven broken edges bevel nicely under pressure, and the narrow strips fit easily. I first place one strip vertically on each side of the joint, previously covered with a layer of cotton-wool; bandage lightly; then diagonal strips; and gently compressing bandage, other strips, and alternating layers of roller, may be added in proportion to the strength required. This lattice-work or diagonal bracing makes a perfectly-fitting and efficiently supporting case, quickly constructed and very easily applied. If the patient is to be removed at once, dry strips of the same material may be fixed externally, but if he can at once be placed in a recumbent position, all that is needed is the support of lateral sand-bags. To be effective, these must be of sufficient weight, varying from twelve to twenty pounds each, and from eighteen inches to three feet in length, according to the joint injured and the patient's strength.

Another patient illustrates in a still more striking manner the principle inculcated in these lectures.

C. H., aged seventy-two years, attended at the hospital on December 7th ult. A short time previously she had fallen headforemost down a flight of stairs, and had sustained a wound on the top of the head two and a half inches long, not exposing the bone. There was also a fracture of both bones of the right forearm at the junction of the lower and middle thirds. That of the radius was a simple fracture, that of the ulna comminuted and compound through two wounds situated on the inner side of the limb, two and a half inches from the wrist. Each of these wounds was three-quarters of an inch in length. They were covered with dry lint pads, as was also the scalp wound; the hand and forearm fixed, in a semi-prone position, in the ordinary wooden splints. Contrary to advice, the patient refused to remain in the hospital. She returned three days afterwards (Dec. 10th), when she was admitted as an in-patient. She complained of feeling ill, and there was considerable pain, redness, and swelling about the head. On removing the lint pad, however, the wound was nearly healed. The head was covered with an oakum pad, and over it a layer of cotton-wool and gently compressing bandage. As there was no complaint of the forearm, it was not disturbed. The patient was put to bed, and a calomel-and-jalap purge administered. Feverishness passed off in a few hours, and the old lady continued to enjoy excellent health and spirits.

Dec. 17th (tenth day).—Splints removed for the first time. The lint over the wounds, which was hard and saturated with blood, not disturbed. No cedema or redness about the limb. Long slough of cellular tissue drawn out from the scalp wound. Dry dressings, with compression as before, applied to the scalp, and the arm-splints readjusted.

21st.—Scalp wound firmly healed. Arm exposed, showing two firm linear cicatrices; not much bony union. Appetite and general health very good. Arm-splints readjusted.

27th.—Scalp cicatrix sound and painless. Forearm firmer.

On Jan. 1st, three weeks after admission, the old lady felt so well that she insisted on going home for the new year. You now see her on the thirty-second day after the accident. The fracture of the forearm is consolidating satisfactorily, but there is a little discharge from the centre of the scalp wound. The scar has yielded a little since the old lady went out, but the surrounding skin is pale and healthy; the scar itself of normal pink hue and painless. The few drops of discharge are quite odourless, and the patient's health is excellent. The dry dressing and bandage reapplied.

Please to recall the extensive wound of the ear and bruising of the temporal region presented to you in a former lecture. That patient was at once taken into the hospital, and, under dry dressing and pressure, union took place directly and without swelling. So I judge from experience it would have been in the case of the old lady, if she had not insisted on going out with two such severe injuries. But even when erysipelas of the scalp had set in, with redness, pain, and swelling about the head, and signs of constitutional indisposition, dry dressing and pressure answered perfectly. Under the old plan of wet applications, whether as lotions, fomentations, or poultices, phlegmonous erysipelas has a terrible tendency to spread, the discharge creeping, inoculating, undermining, and destroying. If you trust to position and rest, to regulation of the circulation by surface pressure and digital compression of main arterial trunks, to efficient drainage, whether by india-rubber pipes or dry absorbing pads, you will have the greatest possible reason for congratulating yourselves on the success of treatment, often under circumstances seemingly the most unfavourable.

I have very rarely seen more extensive injury to a limb repaired with less constitutional disturbance, with more perfect freedom from pain, and less loss of tissue at the seat of mischief, than in the lad before you, whose progress I have simply watched, while the treatment has been conducted throughout by our house-surgeon, Mr. Jordan Lloyd.—*Lancet*, Feb. 8, 1879, p. 181.

ORGANS OF CIRCULATION.

58.—CASES TO ILLUSTRATE THE BEHAVIOUR OF CARBOLISED CATGUT LIGATURES UPON HUMAN ARTERIES.

By THOMAS BRYANT, Esq., Surgeon to Guy's Hospital.

The carbolised catgut, as made by the Apothecaries' Company, Virginia Street, Glasgow, has been so freely employed by surgeons since it was brought before the profession in 1869 by Professor Lister—then of Glasgow, and now of London—that it would seem the time had fairly come for some definite expression of opinion as to its behaviour when applied as a ligature to an artery; for all surgeons should know what they are doing when following any precise line of action, and understand the *modus operandi* of the surgical appliances they use.

It is, therefore, with the view of helping the solution of this question that I have brought forward such pathological and clinical facts as my practice has supplied me with, these facts enabling me to draw certain conclusions upon the point in question. And first of all as to my pathological facts.

Preparation 1, with drawing, represents the external iliac artery of a man who died *fourteen hours* after the application of the ligature to the vessel.

The patient, 29 years of age, was admitted into Guy's Hospital, with ulcerative endocarditis and an aneurism of the right common femoral artery. The aneurism burst, and as the man's life was threatened from concealed hemorrhage, I was induced to apply a ligature to his external iliac artery. The operation, however, failed to save life, and the man died from heart-affection fourteen hours subsequently. In the operation I employed a medium-sized ligature, and did not employ more force than I thought sufficient to occlude the vessel; and what do we find? That the inner and middle coats of the artery are completely divided, and the external coat partially so; that is, it is completely divided in parts. A fair amount of clot exists on both sides of the ligature, and the ligature itself is intact.

Preparation 2, with drawing, represents a right common carotid artery, to which a ligature had been applied *twelve days* previously.

It was taken from a man, æt. 58, who was admitted into Guy's Hospital with a supposed aortic and innominate aneurism, which was pressing upon the trachea and œsophagus, and giving rise to much nerve pain. The operation was performed on January 16, 1877, and a carbolised catgut ligature was employed. The operation was followed by marked relief to pain and other symptoms, but did not save life, the patient dying on the twelfth day from exhaustion.

Now in this case it will be seen that the whole artery at the seat of ligature was completely divided, and the ends separated. There was clot in the vessel above and below the seat of ligature, but it was not firmly adherent. The ligature itself had disappeared. In this case the *left* subclavian artery was found to be completely occluded from arteritis, and other interesting complications existed, which have no bearing, however, upon the question we are now considering, and need not, therefore, detain us.

Preparation 3, and drawing, represents the right subclavian artery, to which a catgut ligature was applied *thirteen days* before death.

It was taken from a man, æt. 36, for whom the operation was performed on February 18, 1877, for a ruptured traumatic axillary aneurism, which was complicated with fracture of the great tuberosity of the humerus.

The man died on the 13th day from lung trouble, the parts about the wound having gone on towards repair in all ways satisfactorily.

At the post-mortem, as made by Dr. Goodhart, over the right clavicle was a linear wound healed in great measure, but leading at one or two places into a sinus-like cavity behind the clavicle. On opening this up a large amount of inflammatory material was found, apparently containing a quantity of bony material in a very early stage of formation, so that it was hardly as yet even gritty. This material ran along in the course of the incision behind the clavicle. There was no suppuration whatever about the wound, all the parts looked perfectly healthy. With some difficulty the subclavian artery and vein were got out of this dense material, and found to be quite normal, except that the former was ligatured just beyond the scalenus anticus. The artery at this spot contained small but firm clots for half an inch above and half an inch below the seat of ligature. On making a vertical section of the vessel, after it had been hardened in spirit, these clots were well seen, and at the same time it became clear that the whole of the coats of the artery had been divided, and repaired. The knot of the ligature alone remained to testify to its presence.

Preparation 4, and drawing, represents the common femoral artery, to which a ligature had been applied *nineteen days* before death.

It was taken from a girl, æt. 23, who was admitted into Guy's Hospital with elephantiasis arabum of the right leg.

The operation was performed on March 27, 1877, and the patient died on the 19th day from gangrene of the limb.

The repair about the wound was good in all ways, very little suppuration having taken place.

The artery was not examined until it had been macerated and hardened.

On making a section of the vessel, all the coats appear to have been divided and to have repaired, and good clots existed above and below the seat of ligature. The knot of the ligature remained in a small hollow close to the vessel, and as it was attached to the artery there is reason to believe that some portion of the loop of the ligature still remains.

Now, it will have been seen, in all these four preparations, that the inner and middle coats of the ligatured vessels have been divided, and it is probable that this division took place as an immediate effect of the application of the ligature. It will also have been seen that the external coat has also been divided: in Case No. 1, within fourteen hours of the application of the ligature, and in parts only—and in the other three, at a later date, by what must have been an ulcerative process, for in Case 2 the whole vessel was completely divided and its ends separated; and in the other two more or less division of the external coat was to be made out. It is clear also that these results were due to the ligature. As a consequence the conclusion is inevitable:

That the catgut ligature, in its *primary* effects upon the ligatured vessel acts as any permanent ligature, and that the two inner coats of the constricted artery are divided by it as a direct result of its application.

But it does more, for, as shown by the preparations, it induces an ulcerative action in the external coat, more or less complete, as a secondary effect.

I will now turn to my clinical experience, and see what evidence I can bring forward to support the conclusions to which the preparations I have brought before you have led me, and I have to report that I have ligatured with the catgut ligature ten other large arteries in continuity, viz.:—

- 5 femoral,
- 4 external iliac, and
- 1 subclavian.

That in one of them the patient died on the tenth day, in two there was secondary hemorrhage, and in the other seven cases an uninterrupted recovery, and, I should add, with little or no suppuration. Indeed, in one case of ligature of the femoral artery, the wound healed by primary union without one drop of pus: the inconvenience of the operation-wound having been so slight that the patient only knew a cutting operation had been performed when he saw the healed scar of the wound on the seventh day, when the dressings were first cleared away.

I should add also that in none of these cases was the spray system followed. Rest to the part operated upon, moderate

pressure over the seat of operation, and dry lint to the wound, were the means employed subsequent to the operation—water dressing with or without some medicated lotion being used when suppuration appeared.

In the single fatal case—that of a femoral artery—the man died on the 14th day from pyæmia and heart disease, and here after death the inner and middle coats of the artery were found to have been completely divided and the external coat ulcerated—the ligature, with the exception of the knot, having disappeared.

In the subclavian case some bleeding from the wound took place on the fourth day, but was arrested by pressure, and by the 23rd the wound had healed.

In one of the four cases of ligature of the external iliac artery the wound, which had almost healed, opened on the 29th day and bled; but pressure succeeded in arresting the bleeding, and all did well.

Now these three cases go to support the conclusions to which a careful examination of the four preparations I have exhibited has led me—for in the single fatal case the inner coats of the ligatured femoral with the external were found to have been completely divided on the 14th day, and in the two other cases mentioned, in which bleeding took place on the 4th and 29th day respectively, this could only have occurred from some ulceration of the vessel before its permanent occlusion had been completed.

I am, therefore, led to conclude: That the catgut ligature in its primary effects, acts upon the coats of an artery as any permanent ligature; and that as a secondary effect it may and does generally excite an ulcerative action in the external coat of the vessel, even to its complete division.

If, therefore, I cannot endorse what the distinguished introducer of the catgut ligature (Prof. Lister) claimed for it in 1869,—‘That by applying a ligature of animal tissue antiseptically upon an artery, whether tightly or gently, we virtually surround it with a ring of living tissue and strengthen the vessel where we obstruct it’—I may express my belief that, as the loop of the catgut ligature dissolves within an uncertain period, and there is not of necessity any such sloughing or ulceration of the whole coats of the constricted artery as must ensue when a more permanent material is employed, we have in the carbolised catgut the best ligature at our disposal.—*Clinical Society's Transactions*, vol. xi., 1878, p. 1.

59.—ON THE TREATMENT OF NÆVUS BY THE EXTERNAL APPLICATION OF SODIUM ETHYLATE.

By Dr. J. BRUNTON, Surgeon to the Royal Maternity Charity, London.

At a late meeting of the Medical Society of London Dr. Brunton read a paper on the Treatment of Nævus by the External Application of Sodium Ethylate, exhibiting cases in illustration. *Case 1.*—F. S., aged seven, had, about a week after birth, a small red spot slightly raised on the cheek, which at about the age of four months had developed into a nævus of the size of a half-crown piece, bluish-red, and raised. It was treated, but with only partial success, by ligature and needles, and when the sodium ethylate was applied the growth was of the size of a crown piece. *Case 2.*—Also a female child, where a nævus was seated over the anterior fontanelle, the size of a shilling. In the first case the original nævus yielded in about a couple of months to the treatment, the continued obliteration of the fresh growth lasting for about a year. Sodium ethylate, which was first obtained and used by Dr. Brunton in 1871, is prepared by adding the metal sodium piece by piece to some absolute alcohol in a wide-mouthed bottle; cautious addition of more sodium until effervescence ceases results in the deposition of a crystalline substance— C_2H_5NaO —at the bottom of the flask. The credit of bringing this substance and other alcoholic and ethylic derivatives before notice was due to Dr. Richardson, who, in a communication on the subject to Dr. Brunton, writes:—"When it is brought into contact with water it is decomposed, the sodium becoming oxidised by the oxygen of the water to form sodium hydrate, and the hydrogen of the water going to reconstitute the common or ethylic alcohol. The change of ethylic alcohol into sodium alcohol transforms it from an irritant to a caustic. Laid on dry parts of the body the sodium ethylate is comparatively inert, creating no more change than the redness and tingling caused by common alcohol; but as soon as the part to which the substance is applied gives up a little water, the transformation described above occurs, caustic soda is produced in contact with the skin in proportion as water is eliminated, and there proceeds a gradual destruction of tissues, which may be moderated so as hardly to be perceptible, or may be so intensified as to act almost like a cutting instrument." Speaking of the practical uses of sodium and potassium alcohols, the same writer says that he does not yet see the means of applying them internally, but predicts for them a very extended application for external purposes, they being most potent caustics—*e.g.*, for the destruction and removal of malignant growths beyond the reach of the knife, by application to the surface, or by subcutaneous injection into the

growths. Applied direct to the unbroken skin, their destructive action is less painful than would be expected, and when pain is felt it may be checked quickly by dropping upon the part a little chloroform, which decomposes the alcohol, converting it into a chloride salt and ether. Dr. Richardson had also found these alcohols to dissolve some of the vegetable alkaloids—*e.g.*, opium; and thus a way may be opened to one of the greatest needs in medicine—a sure, rapid, and painless caustic. “The caustic alcohols may be used in combination with local anæsthesia from cold. A part rendered quite dead to pain, by freezing with ether spray, could be directly destroyed by the subcutaneous injection of caustic alcohol, a practice very important in the treatment of poisoned wounds, such as the wound from the bite of a snake, or a rabid dog. It is by no means improbable that some cystic tumours may be cured by the simple subcutaneous injection of a little of these fluids, after destruction of sensibility by cold. Potassium and sodium alcohol, added to the volatile hydride of amyl, dissolve in the hydride and produce a caustic solution. When this solution is applied to the skin the evaporation of the hydride takes place, and a layer of the caustic substance is left behind. This application was of value to the surgeon.” The author, continuing, said that, compared with the action of nitric acid, there was but little destruction of the epidermis, and he considered that the sodium ethylate acted as an astringent, and the pain was not so severe as that caused by nitric acid. In his cases hardly any scarring ensued.—*Lancet*, Nov. 2, 1878, p. 625.

60.—HOT WATER AS A HÆMOSTATIC IN SURGERY.

By CHARLES B. KEETLEY, Esq., Assistant-Surgeon to the West London Hospital.

That hot water is a valuable means of stopping certain cases of hemorrhage is a fact which I have long observed and frequently turned to practical account. At the same time it is a truth universally known that hot water is a valuable aid to increase the flow of blood after venesection. These statements, though apparently opposed to each other, are unquestionably true.

Having had the good fortune to go to an excellent school where hemorrhagic noses were not infrequent, I first observed its efficacy in many cases of epistaxis, both traumatic and spontaneous. Afterwards, having read a notice of a paper by an American physician (I have unfortunately and ungratefully forgotten his name), stating the *blanching* power of hot water injections into the vagina, I suggested to Mr. Furneaux Jordan that he should use it as a styptic in operating on recto-vaginal

fistula. He did so with entire success in a case treated at the Queen's Hospital, Birmingham, in the autumn of 1875. In February, 1876, I wrote to the British Medical Journal a short note on the treatment of epistaxis by hot water, which probably attracted no one's attention.

In 1876, Dr. Windelband recommended injections of hot water in cases of uterine hemorrhage. Temperature of the water should be from 38° to 39° Reaumur, and in severe cases 41° ($= 117^{\circ}$ to 120° , in severe cases, 124° Fahr.). See Medical Times, August, 1876.

Dr. De Mussy has used Dr. Chapman's hot water bags to the spine for the purpose of checking intra-uterine hemorrhage.

But the first mention which I can discover of the hæmostatic properties of hot water is by Stillé, who, in his Therapeutics, recommends it for leech-bites.

On July 3rd, 1878, at the West London Hospital, having amputated a thigh in the upper third, and there being a great deal of oozing from the stump, especially from a quantity of "nævroid" tissue which the knife had divided, I bathed it with cold water for a long time, but without stopping the bleeding. I suddenly drenched the stump with water of a temperature of 120° . All oozing stopped in a few seconds.

In epistaxis, hot water is most effectual when the bleeding comes on during the morning ablution, with cold water, or during the progress of a cold in the head. But it answers frequently in cases of traumatic origin. It should be combined with elevation of at least one hand above the head; but the combination is not essential. The water need only be applied freely to the face, need not be injected into the nostrils. The head should not be bowed down over the basin more than is necessary. Upon two occasions when I injected hot water into the nostrils I dissolved a large teaspoonful of chlorate of potash in a tumbler of hot water, and injected the solution. The saline was used to prevent swelling of the mucous membrane, which so often follows contact with pure water, especially cold water. This hot saline solution stopped the epistaxis instantly.

I must add, in common fairness, that several friends of mine, as well as myself, have found the hot water powerless against epistaxis of a certain grade of severity, and against hemorrhage from any but quite small vessels.

I could give other cases in which the effect was less evident; *e.g.*, one of bleeding from a wound in the tongue which, after lasting six hours, stopped two minutes after using hot gargles to the mouth. But in this case, styptics, such as Tinct. Ferri Perchlor., had been applied to the tongue only a short time previously, and their action might have been continuing.

How does hot water check hemorrhage? In the case of epistaxis I believe it acts almost entirely as a derivative. It relieves congestion of the bleeding mucuous membrane. In very hot weather, when the warm local bathing threw the patient into a general perspiration, I have known cold water to succeed where hot failed.

But why the direct application of hot water should blanch mucous membranes and sometimes even the surface of wounds, and yet greatly dilate the vascular system in other cases, is difficult to explain. It is worthy of remark that the hands a few minutes after washing in hot water may often be observed to be paler than before. The last phenomenon is not altogether due to the removal of dirt.

On the whole, the most plausible explanation of the hemostatic action of hot water is that its heat *when sufficient* acts as an excitant to the nerves of the muscular coats of the smaller vessels, and perhaps directly irritates the muscles themselves.

It has been suggested that, when used against post-partum hemorrhage, the stimulus acts on nerve-centres situated in the substance of the uterus itself. This action may be direct, or it may be reflex, or it may be both.

Hot water should be used in operative surgery. If it be granted that hot water is as effective as, or more effective than cold as an anti-hemorrhagic, then I think it ought to be preferred to cold during surgical operations. Surely there are few persons who, witnessing a large amputation of the thigh, for example, have not shuddered to think of the depressing effect upon the patient's vital powers of spongeful after spongeful of ice-cold water drenched over the wide surface of the flaps and stump. There is no class of operations which have so rapidly advanced in point of safety as the class of abdominal sections, and one of the chief precautions acknowledged to be desirable in that class is that the peritoneum shall be kept warm. When this precaution is neglected, it has been remarked that all the blood in the body becomes gradually lowered in temperature as successive portions flow through the exposed and refrigerated intestinal and mesenteric vessels. How much this must increase the depressant action of a serious operation is clear, and for similar reasons the surgeon ought not to chill a stump.

The whole subject is very interesting, alike from physiological, pathological, and therapeutic points of view, and I trust that the above paper tends to show that it is full of promise.—*Practitioner*, Feb. 1879, p. 113.

61.—ON A METHOD OF ADMINISTERING CHLOROFORM FOR OPERATIONS ABOUT THE MOUTH.

By JOSEPH MILLS, Esq., Administrator of Chloroform to St. Bartholomew's Hospital.

In cases of operation about the mouth, as removal of the tongue, removal of the jaw, and more particularly cleft palate, the difficulty of maintaining anæsthesia has been a source of much trouble both to the operator and to the administrator of the anæsthetic. With the means usually employed, such as a piece of lint, towel, or face-piece, it is impossible to maintain the anæsthesia uniformly throughout the operation. If any one of these means be adopted, it is necessary that very profound anæsthesia should be produced before the operation is begun; for the administration must cease at the commencement of the operation, and, though the anæsthesia may have been pushed as far as the administrator dare, the patient will soon recover from the effects, and this may and often does occur at the most critical or delicate part of the operation. The struggling occasions great inconvenience to the operator, and necessitates considerable delay; for, when it occurs, one or other of two things must be done—either the operation must be discontinued for a time until anæsthesia has again been produced, or the operation must be continued while the patient is struggling and suffering pain.

In very profound anæsthesia, blood is much more likely to get into the larynx and interfere with respiration than when the patient is only lightly under the influence, although quite sufficiently so for the operation to be performed without producing pain or struggling. Very profound anæsthesia, too, is generally accompanied by more or less syncope.

For operations about the mouth I always use chloroform; because the inhalation of ether often causes much coughing and choking, and is accompanied by a flow of viscid saliva which is most inconvenient to the operator, especially in cases of cleft-palate. Then, too, in the administration of ether, it is necessary to exclude nearly all air, so that it is impossible to continue it during an operation about the mouth.

I have lately made use of an apparatus for the administration of chloroform in such cases as those just referred to, which overcomes the difficulties. The apparatus employed is one which enables the vapour to be injected through a small tube into the respiratory passages either by way of the mouth or nostril.

The chloroform vapour is obtained by means of Dr. Junker's very ingenious apparatus, which is usually employed for the administration of bichloride of methylene only, though it was intended by its inventor for the administration of chloroform

also. For the face-piece, however, which has, so far as I am aware, alone been employed hitherto in connection with it, I have substituted a flexible metallic tube of a size which can be conveniently introduced within the nostril when required. A tube of this kind possesses great advantages, for instance, in such a case as removal of the superior maxilla; when all available space is occupied by the operator and his assistant, the tube can be introduced by the mouth, and interferes with no one.

On the other hand, by any of the ordinary methods there is no opportunity, from the time the first incision is made until the bone has been removed and the hemorrhage has been stopped, for the readministration of the anæsthetic, unless at the expense of just so much delay on the part of the operator.

Again, in a case of removal of the tongue, in which one and often two *écraseurs* form an additional impediment, the tube can be inserted into the nostril and the administration continued. Being flexible, the end of the tube which projects from the nostril can be so bent that it is not in the operator's way.

In using the apparatus the receiver, which is capable of holding two ounces of fluid, is charged with six drachms of chloroform. By pressure of the hand-bellows the air is forced through a tube which leads to the bottom of the receiver and passing through the chloroform, escapes, impregnated with the vapour, through the elastic tube communicating with the flexible metallic tube which is inserted into the patient's mouth or nostril.

With this apparatus the administrator has full control over the supply of the chloroform. The pressure of the bellows should be timed with the inspirations of the patient, and the quantity of the supply is regulated by the quality of the pressure.

I produce anæsthesia at the commencement by sprinkling chloroform from a drop-bottle on to a piece of lint held before the face. When the patient is ready for the operation to be commenced, I throw aside the lint, insert the tube into the mouth or nostril, as most convenient, and so maintain a uniform state of anæsthesia. I have adopted this method since last March, and have employed it in over thirty cases, including two of removal of the jaw and thirteen of operation for cleft palate. In all the cases in which I have used it, except one, it has acted entirely to my satisfaction. The exception was in the case of a man aged forty, whose tongue was removed for epithelioma. He was said to have drunk freely, and he took a large quantity of chloroform and struggled violently before he could be got under its influence. When the operation was commenced, I found that, though by means of the tube I could prolong the anæsthesia, still I could not maintain it, and so was obliged at intervals to resort to the lint and drop-bottle.

I at first used a gum-elastic catheter, which answers very well when inserted into the nostril; but when used for the mouth, I found that it could not be held so steady as a metallic tube, and that, becoming warm, it would not retain the curve I desired.

The apparatus is made by Krohne and Sesemann; the flexible metallic tube was made for me by Arnold and Sons.—*Lancet*, Dec. 14, 1878, p. 839.

62.—ON STRANGULATED FEMORAL HERNIA.

By B. WILLS RICHARDSON, Esq., Senior Examiner in Surgery and in Dental Surgery, Royal College of Surgeons, and Surgeon to the Adelaide Hospital, Dublin.

[Mr. Wills Richardson, after relating some interesting cases of hernia, gives a description of the knife which he uses. He says :]

The knife I used with these cases, I have operated with for some years in inguinal and femoral herniotomy. My first description of it appeared in the *Dublin Quarterly Journal of Medical Science* for November, 1869. I had it constructed with the view to prevent the bowel overlapping the blade in herniotomy. It can be directed to the seat of stricture or point to be incised with great safety, and once within, or under the constriction, it is unerring in its action.

In this knife, the broad back, as now made, is secured to the blade by the anterior end of the latter being somewhat button-shaped, the button passing beneath a ledge in the back. The latter is secured to the blade, posteriorly, with a screw. This back projects 3-10ths of an inch beyond the anterior end of the blade; and laterally, about 2-19ths of an inch at its broadest part. It tapers towards, and terminates near, the handle.

The blade for 6-8ths of an inch anteriorly has a cutting edge that terminates a little behind its greatest convexity. From this point to the handle the edge does not cut. The most convex portion of the cutting edge is 2-8ths of an inch in depth.

In my original model of the instrument the blade and back were in one piece, which I would prefer; but the makers considered that, for facility of sharpening the blade, it would be better to construct it in the manner I have stated.

The knife represented in the figure had only an ordinary spring in the back, but experience in its use proved that it is essential, in order to prevent closing of the handle on the blade, during an operation, that the spring should be provided with a French catch, or some other arrangement for the purpose.

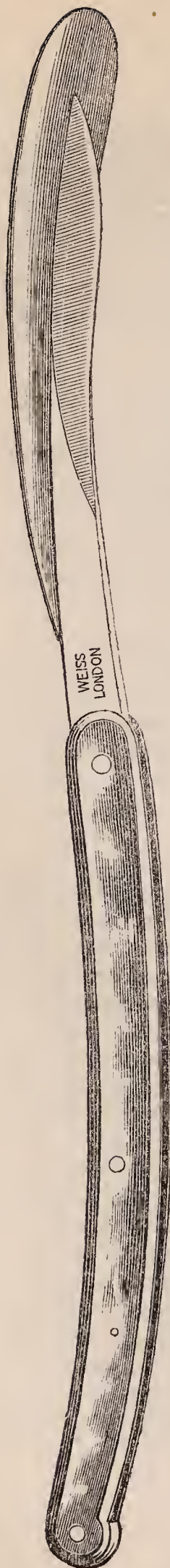
It is almost unnecessary to say that this knife is not suited

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for the division of Gimbernat's ligament. But I believe that greater facilities either for reduction or for relieving constriction are obtained by division of the anterior portion of the crural canal, and if required, of the crural ring, than by division of the ligament of Gimbernat.

To my mind sufficient weight has not been given to the observations of Sir Astley Cooper, on the seat of stricture in strangulated femoral hernia. "Surgeons," he observes, "talk very carelessly upon the subject of dividing the stricture, they say that the finger must be put into the sac, and wherever the stricture is felt that it should be divided. Again, it is said, the stricture is situated on the inner side of the sac, and derived from the pressure of the insertion of the tendon of the external oblique, or what they call Gimbernat's ligament. They do not consider that a stricture is a circle produced in the way as if a cord were tied round the protruded parts, and that the division might be made at any part excepting the posterior, where the bone is placed, if other circumstances did not prevent it. The stricture may be divided on the inner side of the sac, that is, towards the pubes; but to this there are strong objections, which I make the subject of a future chapter. It cannot be divided directly outwards, for there the crural vein must necessarily be injured, and if the division is made upwards and outwards, towards the spinous process of the ilium, the epigastric artery is much endangered. However, this artery is not so liable to be cut in the crural hernia as has been imagined; for it does not approach nearer than half an inch of the upper and outer part of the sac, and an incision of half an inch is more than is usually required to liberate the protruded parts in crural hernia. The stricture may be safely divided upwards, but the mode of doing it, in which there is the greatest security, both as respects the blood vessels, and the protruded intestine, is to pass the bistoury into the middle and anterior part of the mouth of the hernial sac, and to cut the stricture upwards, and with a slight obliquity towards the umbilicus."

Mr. Hey also, as Sir Astley Cooper reminds us, recommended the division of the stricture upwards.



Mr. Aston Key, in an editorial note to Sir Astley Cooper's work, makes the following observations on this important point of practice:—

“In the anatomical description of these parts, I have pointed out what appears to be the usual seat of stricture in strangulation of a femoral rupture, namely, a tendinous band which joins the fascia transversalis to the posterior margin of Poupart's ligament, and which is quite distinct from Gimbernat's ligament, upon which so much stress is laid by some as constituting the stricture. The difficulty which I have witnessed in relieving the stricture when the operator has divided Gimbernat's ligament towards the pubes, and the necessity of repeatedly introducing the bistoury, gives a decided superiority to the upward division of the stricture, which mode of division includes the tendinous band alluded to. Dr. Breschet describes M. Dupuytren's mode of operating as coinciding with our author's as to the parts divided; but he gives a somewhat different direction to his knife which is convex on the edge, and carried obliquely outwards. This mode of dividing the stricture he has long practised, both in the male and female subject, without any hemorrhages. This experienced surgeon concurs with our author and the late Mr. Hey, in the opinion that a very slight incision of the stricture is sufficient to release the intestines; a practice that removes the fears a surgeon may otherwise entertain of wounding the spermatic vessels or the obturator artery, when that vessel arises from the epigastric which by an extensive incision might be endangered.”

Cloquet I am well aware recommended the division of Gimbernat's ligament in ordinary crural hernia. But he mentions that he has seen one case in which the epigastric artery was situated in the inside of the neck of a crural hernia, in a case like which the incision should be made upwards and outwards.

Sir Astley Cooper's further observation on the operation inwards in the “future chapter” to which he alludes, are so important, and not being within reach of a large class of readers, may, without excuse, be reproduced here. They more or less tend to support my view of the value of the knife illustrated above:—

“The division of the stricture inwards, in the operation for strangulated crural hernia, was published by Gimbernat, a Spanish surgeon, in his treatise on this disease. The author having dissected the parts with great care, and finding that the firmest part of the stricture was on the inner side, and that the blood-vessels covered the orifice of the hernial sac on the outer and on the anterior parts (the crural vein and the epigastric artery lying in the former direction, and the spermatic cord on the latter), was led to believe that the operation inwards was

the safest mode of dividing the stricture. If anatomy," Sir Astley remarks, "only directed my judgment I should concur in this opinion; but it appears to me that the danger in crural hernia is not derived from the vicinity of these vessels, but from the chance of wounding the intestine. Nor does this operation entirely prevent even the danger of hemorrhage."

"The objections which I have to urge against this operation are the following;—First, it very much increases its difficulty; for the crural hernia lies buried deep within the thigh, and the orifice of the sac is proportionally difficult to be reached. Even where the stricture is divided anteriorly, the instrument must be passed down to some depth before it can be introduced within the stricture; but if the division is made inwards, from the obliquity of Gimbernats's ligament, the bistoury must be buried so deep as to be entirely obscured by the surrounding parts. Having tried both these modes of operating, I can assure those who have not had an opportunity of making a comparative trial, that the latter operation is much the more difficult to perform."

"Secondly, in Gimbernats's method a danger is incurred of wounding the intestine in two different ways. In looking for the seat of the stricture on the inner side, the intestine (which in crural hernia descends inwards) is obliged to be drawn very much to the outer side, to allow of the finger, or of a director, to be passed to the orifice of the stricture. In doing this the intestine is much stretched at the stricture part, and if it has been long strangulated it readily tears through at this place, and the fæces are discharged at the wound."

This accident may have occurred in a fatal case which he narrates in corroboration of his views on this point of practice; and the description of which he concludes thus:—

"Whether in this case the intestine gave way at the strictured part by drawing it aside, or was injured by the knife, it is impossible to determine, but I was resolved never again to cut inwards, on account of the danger of tearing, as of cutting the intestine."

After narrating a second fatal case in which the operation inwards was performed, and in which at the post-mortem examination, two openings were found in the portion of intestine which had been placed at the mouth of the hernial sac, he proceeds:—

"The other mode in which the intestine becomes injured, is by being cut in passing the knife. The stricture is too small entirely to admit the finger, and the surgeon, fearful of bruising the intestine by attempting to force his finger up the stricture, introduced a director to guide the incision, or the knife without a director; in doing which, a fold of intestine gets before it,

and in the way of the knife, within the mouth of the sac, or still higher, above the sac within the abdomen; but when the incision is made from the anterior part of the sac the intestine is entirely behind the knife. If this accident be immediately discovered, the patient has some chance of recovery with an artificial anus, which may either last only for a time, or continue during life, according to the treatment pursued; but if the intestine is within the abdomen at the time the injury is received, the strangulated portion is returned into the cavity without the wound being suspected, and the patient dies in a few hours, after suffering violent pain, and having the fæces escape through the wound. In such cases, on examination after death, the abdomen is found violently inflamed, owing to the escape of bile into the peritoneum."

"Thirdly, there is another strong objection to the operation inwards, which is, that if the hernia is large this mode of dividing the stricture is not sufficient to allow of the return of the protruded parts, for it gives but little additional room, so that great force must be used to reduce the hernia; and often, after all, the parts behind the crural arch must be divided in the usual way, and the whole operation rendered unnecessarily complex."

"Fourthly, the obturator artery, which, in the usual operation, is only endangered when it varies from its natural course, will be in much greater hazard in the inward incision, as this artery under this variety of course closely embraces the sac on the inner side, but is situated at some distance from its anterior part; and, moreover, if it is cut during the operation inwards it will be scarcely possible to secure it, which it might be, when the anterior incision is performed."—*Medical Press and Circular*, Dec. 4, 1878, p. 453.

63.—CASE OF STRICTURE OF THE RECTUM, TREATED BY EXCISION OF THE STRICTURE.

By Dr. D. LOWSON, Huddersfield.

Mrs. S., aged thirty-four, had complained for eight years of symptoms of stricture of the rectum; and though during that period she had been frequently under medical treatment, and had derived considerable relief, yet the improvement lasted only for a short time, and about two years ago she was suffering more than at any previous period. The motions, which had been narrowed for years, had become much more difficult to pass, and defecation could not be effected without severe straining and considerable pain, and was often accompanied with blood and matter. Hardened masses were felt along the whole course of the colon as far as the cæcum, disappearing

after laxatives and the free use of the enema, and again collecting soon after the discontinuance of these measures. The stricture itself was felt about two inches above the anus, was hard and annular, and at some points ulcerated. It was movable on the coccyx behind, as well as on the vagina in front, and, just fitting the tip of the finger, it could be pushed upwards and drawn downwards freely within the surrounding structures. The symptoms all pointed to a case of simple stricture. There was no great irregularity of surface, and, after an existence of eight years, only a small extent of bowel was affected. There was no excessively fetid discharge, as is the case in most syphilitic or cancerous strictures; and, in addition, the patient was not suffering constitutionally, the appetite and general health being good.

Finding that in the course of former treatment dilatation had not been resorted to, and having first cleared away the masses of scybala filling up the large intestine, a process which I found considerably dilated the stricture, I gradually completed the dilatation by bougie. She was for the time relieved, but a month afterwards I found the stricture as before. The bougie was again resorted to, but the irritation caused by it became so great that its use had to be discontinued. Mild laxatives and frequent emollient enemata soothed and relieved the irritated bowel, but dilatation could not again be borne, and the symptoms of stricture became aggravated.

For twelve months the patient had been under treatment without any marked improvement in the condition of the stricture, and she now became very anxious to have something done for her permanent relief. Dilatation having proved unsuccessful, the idea of colotomy presented itself, but under the circumstances it seemed rather an extreme measure; and excision of the lower part of the rectum, although strongly advocated by high authorities for cancer, has the disadvantage of destroying largely or entirely the action of the sphincters. The removal of the stricture alone seemed the most feasible operation. For, by cutting out the narrowed ring, and stitching together two pieces of bowel which were healthy and had sustained no loss of substance in their circumference by the ulcerative process, a union might be expected free from contraction; and approaching the rectum from behind by an incision extending from a little behind the anus to the tip of the coccyx, and by keeping as near as possible the mesial line, so as to run parallel with the fibres and divide into halves that part of the external sphincter which lies between the anus and coccyx, its action would not be ultimately interfered with, and the internal sphincter would be preserved entire. Mr. Jessop, of Leeds, who saw and examined the case minutely, considered it a very

favourable one for operation, as did also Mr. Knaggs, of Huddersfield. Accordingly, on the 5th of December, 1877, having cut down in the mesial line in the interval between the coccyx and lower end of the bowel, I divided the posterior part of the external sphincter as much as possible into two lateral halves, and turning these aside with the intermingling fibres of the levator ani, I introduced the finger inside the rectum, and pushing it firmly into the stricture I pulled it down from its situation in front of the coccyx, and made it project backwards through the external wound. Reaching the wall of the rectum, and having dissected the surrounding structure from its lateral aspects as far forwards as the recto-vaginal septum, I cut the bowel through above and below the stricture, dissected the ring off the posterior vaginal wall, and stitched the two pieces of bowel together with catgut sutures. Two small vessels spouted, but did not require ligation.

After the operation the temperature gradually rose, reaching its maximum— 102° —on the evening of the third day, and, falling again, became normal two days after. The pulse corresponded with the temperature, being 120 the third day after the operation. The catheter had to be used for a fortnight. There was never any abdominal tenderness nor other symptom of peritonitis. The vaginal pipe of an ordinary Higginson's enema was introduced into the rectum on the conclusion of the operation, and the bowels were kept confined for five days. After this, however, diarrhoea came on, and the management of the wound became difficult. A small-sized Ferguson's speculum was introduced in place of the vaginal pipe, and through this the bowel was cleansed. The stools now became liquid and very irritating, were mixed with small scybala, and came away partly by the tube, but also by the wound, excoriating the integument in its vicinity. Opium had to be prescribed freely on account of the pain, and yet the nights were restless and the appetite became poor.

About three weeks after the operation the lower fragment of bowel gave way behind, probably from the continued pressure of the speculum, and immediately all the symptoms began to improve. There was now no pain except when the bowels were moved; there was considerable retentive power except when the bowels were relaxed; the discharge became less irritating; the excoriations healed; and the blue line began to appear at the margin of the wound.

March 9th, 1878.—The condition of Mrs. S. has greatly improved. The bowels now act regularly; there is neither pain nor straining at stool; the motions are natural in size, but flattened; and the sphincter power is good except when the bowels are relaxed, when she finds retention is not so perfect as formerly.

March 31st, 1879.—A considerable amount of cicatricial structure has formed around the seat of the operation, and some contraction has taken place, but a medium-sized bougie passes easily, and the motions without difficulty. The symptom complained of most is “painful sitting.” To sit comfortably she is obliged to lean well forward, or inclined to one side in a semi-recumbent position. Except during an attack of diarrhoea which she is sometimes subject to, her sphincter power is perfect.

The great difficulty in the case was the after-treatment. The passage of fæcal matter of a very irritating nature over the wound, and the tendency to diarrhoea common to most rectal operations, retarded the healing process, which seemed also to be delayed by the action of the internal sphincter, just as in the cases of fistula. There are few strictures situated so low down as to come within the range of the foregoing operation; but in cases of the sort I think that it might be advisable to perform colotomy in the first instance, so as to carry off the fæcal matter by the loin; then, after an interval, the stricture in the rectum might be removed, and a good union secured, and subsequently the artificial anus closed and the motions allowed to pass off by their former channel.—*Lancet*, April 12, 1879, p. 512.

ORGANS OF URINE AND GENERATION.

64.—ON LITHOTRITY AT ONE OR MORE SITTINGS.

By Sir H. THOMPSON, Surgeon Ext. to H.M. the King of the Belgians; Consulting Surgeon to, and Emeritus Professor of Clinical Surgery at University College Hospital.

After describing the lithotrite, and the manner of employing it, the lecturer proceeded:—

You may naturally ask, “How often is it right to introduce the lithotrite at a single sitting? Is one sitting sufficient for the removal of a stone; and, if so, how often may the lithotrite be introduced, and how long should the proceedings occupy? If not sufficient, how many sittings are necessary or permissible, and what should be the interval of time between them.”

The answers to these questions, which involve several considerations, may be given together.

And, first, it is obvious that, as regards the frequency with which the lithotrite may be introduced, the experienced and dextrous operator may safely use it much more freely than one who is unpractised.

Secondly, if you operate without an anæsthetic agent, you

will find that some patients can bear two or three applications of the lithotrite better than others can bear one. But if ether be used, it is quite certain you may remove much more calculous material than without that agent; and, as this result is a great gain for the patient, it is almost always better to attain it. At the same time operate with as much care and delicacy as if your patient were in possession of his senses. Nothing trains the hand so well as employing the lithotrite now and then for a patient who is conscious of what the operator is doing.

Thirdly, it is very desirable to remove the entire stone at a single sitting if prudent to do so, as it leaves the bladder emptied of fragments, and therefore free from a fertile source of irritation. And such has for a long period been my invariable practice, whenever I have thought it possible to effect my object without injuring the bladder by too much or by too prolonged instrumentation. I have often been able to empty the bladder at one application of the lithotrite, aided by that useful instrument the aspirator of Clover, the patient being under the influence of ether. But calculi vary greatly in size, and patients vary as much in liability to those sources of danger which are more or less associated with all instrumental contact. It is a matter of common notoriety to every experienced surgeon that many persons receive an unfavourable impression, and sometimes a dangerous shock to the system, when large metallic instruments are introduced into the bladder, and still more when they are repeatedly and rudely used there. When, therefore, I have used the lithotrite and aspirator from four to six minutes—a time which amply suffices to remove a uric acid calculus of moderate size—I prefer, as a rule, to postpone further proceedings for a day or two—rarely longer—and to finish the operation at a second or even at a third sitting, beyond which, for some years past, I have not often found it necessary to go. The sum total of the times devoted to such an operation will be at the most twelve or eighteen minutes, ample for the removal of a uric acid calculus weighing four or five drachms, and comprised within a period of from eight to ten days. Mostly the time occupied by a sitting is less than five minutes, and when I operated, as I formerly often did, without an anæsthetic agent, I rarely exceeded two minutes, when, of course, more numerous sittings were required; but since ether has again come into vogue, I have always availed myself of it, and have made the sitting more productive in consequence.

It has been recently proposed by Professor Bigelow, of Harvard, U.S., to make the rule absolute to remove at one sitting an entire stone, no matter how large it may be or what may be the condition of the patient. Invariable conformity to such a rule, I do not hesitate at the outset to say, will lead to results

which, although often successful, will not seldom be disastrous.

Let me state that I have never doubted for an instant that, so far as mechanical power is concerned, almost any stone may be thus removed, and without much difficulty, but I cannot overlook the fact that the vital conditions under which we are compelled to work must limit the employment of mechanical force. In the practice of Bigelow's method, very large and heavy lithotrites are introduced, certainly larger than an ordinary urethra will admit without using force. On seeing them for the first time, I could not help remarking, "Surely these are some resuscitated relics of the early history of lithotrity!" reminding me as they did very forcibly of the terrible engines used by Heurteloup between 1830—40. But what strikes me after all as the most remarkable fact, judging from the very slender experience by which the proceeding is at present supported, is the enormous time which has been consumed with these instruments in performing the task proposed. Thus we find in Bigelow's work, that the duration of a single sitting to remove a stone of less than two drachms was an hour, and for one of less than three drachms an hour and a half! Now, as already stated, the utmost time I ever devote to such stones, and with my small light instruments, amounts to twelve or eighteen minutes, but in two or three sittings of five or six minutes each, all comprised within a period of seven to ten days.

But I am free to confess that the proposal to remove a large hard stone at one sitting is an attractive one. So far from opposing it, I am predisposed to regard favourably any plan by which we may hope to take away, once and for all, the hard and angular fragments which must remain, and sometimes to a considerable extent, after an incompleting sitting. I fully agree with Bigelow that their presence constitutes the chief source of mischief in lithotrity as mostly practised. I only fear whether we may not, *by adopting the system under consideration*, pay too high a price for the purpose of attaining the end proposed. And, in reference to this, I am bound to say that my own system has for a long time past been gradually inclining to the practice of crushing more calculus at a sitting and removing more débris by the aspirator than I formerly did. Thus I have, during two years at least, been in the habit of using in every case two lithotrites alternately (my comparatively small, but strong, flat-bladed instruments); handing the first, when withdrawn, full of débris, to my assistant, who clears it out completely while I am crushing with the other, which in its turn is cleared and again used. Each is probably introduced three times at least, while clogging of the blades with débris is prevented by the clearing process. With these light and handy

instruments, which pass with the utmost facility, employed in this manner and followed by the aspirator, I am quite certain that I can remove calculous matter from the bladder more safely and much more rapidly than with the enormous and unwieldy instruments referred to.

I have moreover taken a hint from Bigelow's aspirator, and, slightly modifying Mr. Clover's original instrument, have, I think, rendered the latter more powerful and perfect, while I have avoided some material disadvantages attaching to the former. The new one, in fact, combines the best qualities of both. Thus I have greatly shortened the channel between the bladder and the aspirator, by getting rid altogether of the long arched tube which enters the top of the American instrument, and making the end of the evacuating catheter enter directly, without curve, at the bottom; so saving many inches of the route which has to be traversed by fragments. The new instrument is very easily filled with water, its action is extremely powerful, no air can possibly enter during the process of using it, and the amount of *débris* withdrawn is at once taken out of the current, and remains undisturbed and visible to the operator throughout the proceeding. I am quite satisfied with evacuating catheters No. 16 in size, English scale (No. 26 French); larger than those are mostly dangerous, and wholly unnecessary. There should be several of them, with openings and curves of different kinds, one variety acting better for one patient and another for another.

There is a direction for the successful use of the aspirator which Bigelow in his detailed instructions regarding that matter has not alluded to. And I do not hesitate to say that the recognition of the fact I refer to is not less important than all the information which can be obtained by observing the action of the aspirator-currents on fragments in an artificial bag, or even in a human bladder after death, useful to a certain extent as I admit that to be.

I contend that a very important rule in employing any aspirator, in order to ensure the minimum of risk with the maximum of efficiency, is strictly to subordinate its action to the respiratory movements of the patient, especially when these are full and deep, as they are apt to be sometimes under the influence of ether. When the respiration is light and tranquil, the rule is less important. Whatever the position in which you may place your instrument, for effective use it is desirable to make the exit of the fluid from the bladder coincide with the act of inspiration by the patient, since the effect of a full expansion of the lungs is as powerful to remove *débris* from the bladder as is the exhausting force of the india-rubber ball itself. In illustration, how often has one observed, when an open silver

catheter is lying in the bladder of a supine patient, that a jet of urine is propelled to a considerable distance by a full act of inspiration. In using the aspirator, then, I let every movement of the hand holding the india-rubber ball conform to the action of respiration, filling the bladder with the patient's expiration, and gaining the force of his inspiratory effort by simultaneously permitting the expansion of the exhausting ball. Indeed, it is only safe (under ether) to inject the bladder during expiration, while the aspirator is only continuously productive during inspiration. By practice the hand of the operator and the respiratory efforts of the patient work together so harmoniously that, as with myself from long habit, it becomes almost impossible for them to act otherwise. It may be as well to add that the short quick act of expiration which constitutes "cough" is, on the contrary, a powerful expelling agent, and when it occurs, as it not unfrequently does under ether, should always be associated with relaxed grasp on the aspirator, and consequent outflowing current.

Since writing the above, I removed, on January 21st, 1879, a hard uric acid calculus, in a sitting of eight minutes, with the two light lithotrites, in the manner described above, and with the aspirator, the débris weighing, when dried, two drachms—a weight exceeding that of the calculus reported by Bigelow as occupying one hour for its removal with his instruments. The patient, aged sixty-nine years, was brought to me by Mr. Lathbury, of Finsbury, who was present, as were also Mr. Clover, who gave ether, and two other medical men. The bladder was entirely cleared; scarcely any blood was seen; no fever followed, and the patient is doing extremely well. I regard this example as an admirable illustration of the capability of the existing lithotrites and of the existing method; the application of the latter, I am ready to confess, having been extended in point of time beyond the limit which I formerly considered prudent or practicable, and which I still consider to be so only in tolerably practised hands.—*Lancet*, February 1, 1879, p. 145.

65.—REMARKS ON LITHOTRITY AT ONE SITTING.

By WILLIAM CADGE, Esq., Senior Surgeon to the Norfolk and Norwich Hospital.

When I first became aware of the views of Prof. Bigelow on the treatment of stone in the bladder by rapid lithotritry and large instruments, I confess that they seemed to me crude in theory and likely to lead to dangerous results in practice. A more careful examination of the subject and a perusal of his excellent paper have induced me to modify my first impressions, and to think that in the new method it may be found that we

possess, not only a novelty, but a real advance in practical lithotrity.

In common with all English lithotritists, it has hitherto been my habit, at the first sitting, to do but little with the instrument (not knowing what the tolerance of the bladder would be), and at subsequent sittings to act more freely. I have, of course, met with cases, though they are rare, in which one sitting has sufficed to bring about a cure; more frequently there have been five or six, sometimes ten or twelve, and occasionally fifteen or even twenty sittings,—depending in great measure on the size of the stone and the power of the patient to void the *débris*. In old persons with a weak bladder and enlarged prostate I have generally introduced the lithotrite two, three, or four times at a sitting; and in some cases of chronic retention of urine combined with calculus it has been necessary to remove as well as crush the whole of the stone. In doing this I have sometimes used Clover's syringe, but more frequently have trusted to the quicker and less disturbing action of the scoop lithotrite.

Speaking generally, the chief principles of English lithotrity, as taught by Brodie and subsequent writers, are—1, to do as little as possible with instruments, especially at the first sitting, leaving the extrusion of the fragments as much as possible to natural efforts; and 2, to use small rather than large instruments. Bigelow's method, as a leader-writer in the *Lancet* has said, is based on opposite and contradictory principles. He proposes—1, to do as much as possible at the first sitting, and, in all but very large stones, thoroughly and at once to clear the bladder; 2, to use the largest instruments for this purpose which the urethra will allow. Both these propositions are new to English lithotrity. He argues that the average bladder and urethra are not extremely susceptible, and that more harm is done by leaving sharp angular fragments to worry the mucous membrane than by the prolonged use of smooth-polished instruments in removing them. In evidence of this tolerance of the bladder, he refers to the generally favourable results of lithotrity; to the success in the treatment of urethral stricture by forcible dilatation and laceration; and to the quick recovery of the bladder from the bruising and injury inflicted during the removal, by lithotomy, of large or rough stones.

The use of large instruments in crushing the stone, and especially in evacuating the fragments, is quite as great an innovation as the principle of the comparative harmlessness of long sittings. I have hitherto felt that large instruments, whether for crushing or evacuating, are more apt to produce rigors, fever, and cystitis than small ones, and it is probable that this view is correct when ether is not used. With full

anæsthesia, however, Bigelow maintains and has found that no harm comes of using instruments equal in size to about 16 to 20 English scale, and it is certain that, without these large evacuating catheters, lithotrity at one sitting on his plan will not be practicable for full-sized stones.

The question then for solution is, which principle is correct and most generally applicable—short first sittings and instruments of moderate size, or thorough clearance of the bladder at one sitting by means of large evacuating tubes? I do not now propose to give any decided opinion; time and experience can alone decide the question, and as a contribution helping to this end I wish briefly to report a few cases in which I have recently adopted Bigelow's method.

Case 1.—F. R., aged thirty-one. Stone of about two years' growth; it measured an inch and a half in one, and an inch in another diameter. Urine and bladder healthy; can retain water from one to two hours.—February 27th: Under full etherisation, I removed the whole stone at one sitting, using two of my ordinary unfenestrated lithotrites by turns, and a No. 16 evacuating catheter. The lithotrites were used alternately nine or ten times, and the evacuating tube twice. The time occupied was about half an hour. The quantity of lithic-acid stone weighed, when dried, 240 grains, of which 180 grains were removed by the tube and 60 by the lithotrites. Scarcely any bleeding accompanied the proceeding. I took care, as I always do, to keep the patient covered, both body and limbs, with thick flannel during the operation, and at the conclusion a half-grain morphia suppository was placed in the rectum. No rigor or trouble of any kind followed; he passed a few fragments the next day which had escaped detection, and on the fourth day he had lumbar pain and a rise in temperature, which subsided on the fifth day, when he voided an entirely fresh stone, which had probably just escaped from the kidney or ureter. In a few days more he was quite well.

Case 2.—J. R., aged sixty-one, a stout, heavy man, with an irritable bladder, so irritable that I had almost resolved on lithotomy, although the stone was not large, but the success of the previous case encouraged me to repeat the experiment.—March 2nd: Under ether, and in a quarter of an hour, I removed the whole of a stone, which measured about an inch. The same instruments were used. The lithotrites were introduced five or six times, and the evacuating tube twice. The dried fragments of stone which were saved weighed 80 grains, of which 52 were removed by the lithotrites, and 28 by the catheter. The patient had no bad symptoms; he voided no more stone; got up and walked about the next day, and was quite well in a day or two more.

Case 3.—Mr. D., aged sixty-six, a farmer from Suffolk, a very nervous timid man, grey-haired, but stout. His father died after lithotripsy many years ago, and several members of his family are subject to stone or gravel. His urine is healthy, but his bladder is very irritable, and he micturates with great pain every hour or hour and a half. On first sounding, without ether, the stone appeared to be of a magnitude to require lithotomy. He could not, however, bear the full use of instruments. On March 12th I therefore gave him ether, in order to measure the size of the stone accurately. Finding it not exceeding an inch and a half, it was at once crushed and removed, as in the other cases. The lithotrites were used six or eight times, and the evacuating catheter thrice, after which I could detect no fragment with a sound or lithotrite. The sitting lasted about half an hour. The stone was lithic acid, with a thin covering of phosphates, and weighed 133 grains when dried, of which forty were removed by the lithotrites and ninety-three by the catheter. There was rather more bleeding than in the other cases. A morphia suppository was used, and he was ordered chiefly a milk diet. Retention of urine followed, which required the catheter every three or four hours for a few days. He was feverish, and had slight cystitis and considerable pain about the root of the penis. About a week after he again took ether, and a few small fragments were crushed and removed with the lithotrite alone. This was not followed by retention, and his bladder was easier, but for some time he continued to experience bladder irritation and urethral pain during micturition.

Case 4.—Mr. C., aged seventy-four, a large rubicund gentleman, with two or three small stones, and increasing vesical irritability. He has long suffered from lithic gravel and small stones, but of late, owing, as he thought, to the use of rain water, no gravel has been passed. It was rather that as his age increased so did his prostate gland; the out-flow of urine became somewhat impeded, and calculi, which used to escape easily, were now detained in the bladder. On March 13th, under ether, and in about ten minutes, two or three small uric-acid stones were broken and removed by the same instruments as in the former cases, in all thirty grains; but in this, as in the other cases, there was a considerable amount of fine sand and pulverised stone, which was lost. A little cystitis followed, but in a week's time he was well.

Case 5.—Mr. W., aged sixty-one, a large robust man, with a small stone and symptoms of moderate severity.—March 20th: Under ether, and in a few minutes, the bladder was cleared of a lithic stone which weighed between twenty and thirty grains. In this case the orifice of the urethra was small, and required to be slit up for half an inch in order to extricate the lithotrite

filled with stone, and to allow No. 14 evacuating catheter to enter. He had no trouble afterwards, and was well in a few days.

In these cases I used only the ordinary unfenestrated lithotrites, and the smallest of Bigelow's catheters. It was easy to recognise the advantage of the full-sized evacuating catheter, and to see that the larger the catheter the greater will be the rapidity and facility in removing fragments. Further experience will, perhaps, enable me to overcome the prejudice against these large instruments, and at the same time to manipulate them more deftly; in the meantime, I think the new plan should not be attempted by anyone who has not already acquired by plentiful experience on the living, and by repeated experiment on the dead body, all the little knacks and tricks which go far to make up successful lithotrity.

The behaviour of the bladder towards a stone in it is most peculiar and puzzling. In one case it displays almost perfect indifference, and even acts the part of host with an approach to hospitality, and allows the guest to stay and grow until it reaches enormous proportions. Some years ago I made a post-mortem examination on an old man dead of abdominal aortic aneurism, and discovered accidentally that the bladder contained a large lithic stone which weighed nearly nine ounces. The bladder was healthy, and the mucous membrane pale. For years I had occasionally attended this patient, but so little did this large stone trouble him that neither he nor I was aware of its presence. In other cases, no sooner does a stone enter at one portal than the conflict begins, and goes on until either it is turned out along the urethral passage at the other portal, or the bladder is perpetually fretted, worried, and inflamed until either the surgeon or death comes to end the dispute. So it is under lithotrity; it is impossible, or nearly so, to predict when inflammation and trouble will follow a sitting. Oftentimes it comes on when the stone is small, the patient healthy, and the manipulations have been most gentle and easy. In other cases the roughest handling under adverse circumstances is borne with impunity. It is this uncertainty which has hitherto made me careful to do but little at the first sitting. But it may be that the boldest course is the safest; and it must be admitted that when cystitis does follow the first sitting, it is a comfort to feel that by the new method there are no sharp fragments too perpetuate the irritation and retard recovery. Cases such as these I have reported, and a study of those related by Bigelow, will go some way to win a place for this new method in English practice. What that place may be—whether it will extend the limits of lithotrity, and, if so, to what extent; what modifications of it and improvements in it are possible,—these are

points which further experience will decide. In the meantime I cannot forbear expressing my high appreciation of the work which Professor Bigelow has already accomplished, and of the fair, candid, and forcibly written essay in which he has advocated his views.—*Lancet*, April 5, 1879, p. 471.

66.—ON THE IMPORTANCE OF AN EARLY DIAGNOSIS OF STONE IN THE BLADDER, AND THE MEANS OF EFFECTING IT.

By W. F. TEEVAN, Esq., B.A., Surgeon to the West London Hospital; Surgeon to St. Peter's Hospital, &c.

Luckily, a stone is a long time growing to a considerable size, and it usually gives a good warning of its presence, so that ample time is afforded to attack and crush it while it is yet young, with absolute safety to the patient. Everything, consequently, turns on the diagnosis of the complaint. It may be said that the risk attached to stone in the bladder depends on its size; the larger the calculus, the more disease it occasions, and the greater the risk, therefore, attached to its removal. A big stone means much disease and great risk; whereas, if a small one, it may be affirmed, little disease, small risk. Hence it ought to be our aim to discover a calculus while it is yet young, and to crush it in its infancy. Now, the symptoms which should lead us to suspect the existence of a stone in the bladder are often obscure and ill-defined when taken singly, but, if grouped together and subjected to the crucial test questions which I shall hereafter refer to, we get together a mass of evidence which is usually strong enough to enable us to say whether a patient be the subject of stone in the bladder before we sound him.

A stone in the bladder is a foreign body lying unattached, and the symptoms of calculus are chiefly the results which would be produced by any foreign body shifting its position every hour through movements impressed on it. Every other affection is a fixture, but this one is not; consequently the symptoms are greatly exaggerated by movement and relieved by rest. This cannot be affirmed of the other complaints. If therefore, you have a certain set of symptoms presented, and if they be aggravated by movement and relieved by rest, depend upon it that you have a stone to deal with. Let me take the case of a man suffering from frequent, difficult, painful, and bloody micturition, all of which symptoms may be caused by an enlarged prostate which has become inflamed. You want to clear up the case. All you have to do is to ask the man whether he is worse at night or in the day. If it be a prostatic case, the man will tell you that he is just as bad at night as in

the day, or even worse; whereas, if it be a stone-case, the patient will say to you that he is not so bad at night, for the simple reason that the absence of movement brings relief. Take another case. A man complains of frequent, difficult, painful, and muco-purulent micturition, all of which symptoms may be caused by a bad stricture. Ask him if he is better or worse when he moves about or rides in an omnibus. If he suffer from stone, there will be a great exaggeration in the symptoms; whereas, if it be stricture, there will not be.

I have enumerated so many symptoms that they may possibly be difficult to recollect. If, however, you can remember the words *pain* and *blood*, and that both are *increased by exercise*, you will rarely ever overlook a case of stone; for, depend upon it, if a man come to you and complain that he suffers pain and passes blood when he passes urine, and says that he is much worse when he rides, you may at once be certain that you have a calculus to deal with.

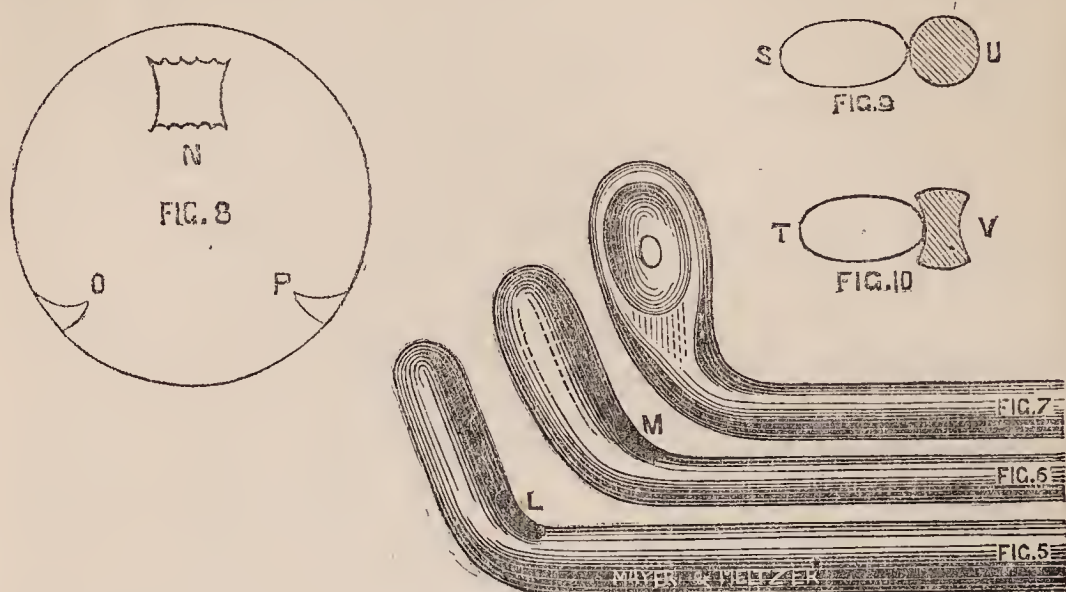
One word for encysted calculus. Luckily, it is very rare; and, as it is a fixture, the ordinary symptoms would be conspicuous by their absence. The patient would be only troubled by increased frequency of micturition, and his urine would be cloudy.

So much for the occurrence of symptoms which would lead us to suspect the presence of a stone in the bladder. It is, of course, absolutely necessary to confirm our suspicions by sounding the patient, for by that process shall we alone be enabled to declare with certainty whether a patient has a stone. Now, sounding a patient is not a manipulation to be lightly undertaken, or performed at any time under any conditions. Cases are on record of patients who have died after being sounded, apparently from peritonitis set up by inflammation extending from the bladder to the peritoneum. Hence care ought to be taken to see whether the patient be in a fit condition or not; for, if he be at all feverish, the operation had better be deferred. It must, however, be borne in mind that it is much better to examine twenty patients unnecessarily, rather than allow a single case of stone to slip through the surgeon's hands for want of an examination. The patient's bowels ought to have been thoroughly opened, as a loaded rectum interferes with the examination; and care ought to be taken that there is not too much water in the bladder, for, if there be, the urine will be driven out along the sides of the sound as soon as the instrument is introduced. Therefore tell the patient it will suffice to have a couple of hours' urine in the bladder for the purpose. He ought to be laid down on a hard couch, with a good stiff bolster under his buttocks; but the head and shoulders ought not to be supported by any cushion. The

surgeon had best stand at the bottom of the couch between the patient's legs, for in this position only can he thoroughly examine the rectum and bladder at the same time, as is absolutely necessary. If the patient be told to draw up his knees, and to place each foot on a chair, he will have placed himself in the most advantageous position for the surgeon. Some of the sounds employed in this country are fitted to do anything except what is required of them—to detect a stone: and, if it has been my good fortune to detect many calculi which had eluded the search of hospital surgeons, I would attribute it to the fact that I have used a better sound than they did, for which I am indebted to M. Mercier, and which is depicted in Fig. 5. It is hollow and has its eye (L) in the right place—the concavity—where it is less likely to be plugged up by mucus or to annoy the patient than if placed in the convexity, as is to be noted with most English sounds. My favourite sound is to be seen at Fig. 6. It differs only from the preceding in having a larger beak and a thinner shaft. The larger the beak the more capable it is of detecting a calculus, and the more slender the shaft the less irritation will be caused the patient by the manipulation of the sound in the urethra. But the peculiarity of Mercier's sound is the shortness of its beak and the sharp angle at which it is fixed to the shaft. The beak is from a quarter to half an inch shorter than that of any other sound in use in this country, and the special advantage gained is that it has a more extended field of exploration allowed it than the longer-beaked sound, and, above all, that it alone can efficiently examine that depression which exists behind an enlarged prostate and is the favourite home of stone. The nearer the angle formed by the beak with the shaft approaches a right angle, the more efficient will the sound be; but, as such an angle would not permit of the introduction of the instrument, Mercier has fixed his beak at an angle of about 110 degrees. It might at first sight be thought that a sound such as I have described would be difficult to introduce; but it is not so. To pass it easily, the handle must be made to describe a much greater portion of the circumference of a circle than the ordinary sound; so that, when the introduction is commenced, the handle ought to be laid on the abdomen, and the turning movement delayed as long as possible; that is to say, the sound ought to be allowed to find its own way as far as it will go before the surgeon attempts the turning movement. When, however, he commences that alteration in direction, he will have to continue it for a longer period than with the ordinary sound, and to depress the handle almost to the couch before the instrument enters the bladder.

Another sound of Mercier, quite unknown in this country, is

that represented by Fig. 7. It every now and then happens that, although the existence of a stone may be strongly suspected, yet it cannot, on examination, be found, usually on



account of its small size. Now, as a rule, the opposed surfaces of a stone and sound are always convex, as seen at Fig. 9, which represents sections of the stone *s* and the sound *u* in apposition. It will be observed that they only touch at a single point, and hence the percussion-note produced is extremely feeble, or possibly inaudible. Then, again, inasmuch as the beak of the sound is convex, it tends, when it strikes a small stone, to push it away, and it may not again be found. To remedy these defects, and to facilitate the detection of a very small calculus, Mercier constructed a hollow sound (see Fig. 7), the beak of which is flat and spoon-shaped on each side, so that, when it strikes a small stone (see sections of stone *t* and sound *v*, Fig. 10), it overlaps or embraces it; so that, instead of the two bodies being in apposition at *one point* only, they each present to the other a *surface*. Thus, therefore, a note which may be inaudible when two bodies are struck, and touch at one point only, becomes very distinct when surfaces meet. However, such a sound is not often required. The female sound ought to be discarded; it is well-nigh useless. The surgeon who uses it cannot carry out any systematic exploration of the bladder, for he can only poke about the viscus with it. The instrument, indeed, resembles a small poker. When the sound, such as *I* have represented at Fig. 6, has been passed, the surgeon ought, firstly, to explore the right side of the bladder, then the left; next the suprapubic region, then the floor; and, lastly, he ought to reverse the beak, in order to examine the depression behind the prostate, which is the favourite haunt of calculus in

middle life and old age. It is well known that, on several occasions, patients have been cut for stone and none found; and it is, therefore, well to bear in mind that there are three separate and distinct bony points which may be struck in sounding, any one of which may give rise to the belief that there is a calculus. The three points are: 1, the sacrum (N, Fig. 8); 2, the spine of the right ischium (O); 3, the spine of the left ischium (P). When an inexperienced surgeon introduces a sound into the bladder and strikes a something, he at once thinks it must be a stone, not reflecting that things *extravesical* as well as *intra-vesical* may be felt. The bony points to which I have referred are felt *through* the walls of the bladder, and hence, when struck, give forth a dull indistinct sound, very different from the click from a calculus. No careful surgeon, however, would ever operate on a man without having previously passed a lithotrite into the bladder, which clears up a case and prevents any error from being committed. The operation of sounding cannot be conducted too slowly, even to the withdrawal of the instrument; and the process of "whipping" the sound out of the bladder cannot be too strongly reprobated, as one fraught with much danger to the patient.—*British Medical Journal*, Dec. 28, 1878, p. 947.

67.—ON THE INTRODUCTION OF THE SOUND WHEN IT CANNOT BE PASSED.—A SOLUTION OF THE PARADOX.

By W. F. TEEVAN, Esq., B.A., Surgeon to the West London Hospital and St. Peter's Hospital.

It occasionally happens, on account of a great enlargement of the third lobe of the prostate, or the engorgement of one of its lateral lobes only, or the formation of a valve at the neck of the bladder, or the existence of a tumour of a malignant nature, that, although an elastic catheter may be passed into the bladder, the introduction of the metal sound is an impossibility through the tortuosity of the canal. In such a dilemma English surgery offers no resource; and, although it may be of the utmost importance to pass a sound to effect a complete exploration of the bladder, that organ would, under such circumstances, remain an unexplored region were it not that a French surgeon has by a simple combination shown how the difficulty may be overcome. A celebrated surgeon being unable, after several attempts, to pass a sound on a gentleman, the patient sought the advice of M. Mercier, who, being equally unsuccessful, put into execution a plan which he had previously devised for such an emergency. The vesical extremity of a large soft catheter having been cut off in such a way as to leave a rounded end, a slit, about one inch long, was made on the con-

cavity of the tube, and a strong ferrule fixed on the other extremity. Some lard or wax having been put into the slit, so as to make the end smooth, the tube is introduced into the bladder. A long, slender sound, consisting of two portions respectively thirteen and twelve inches in length, and capable of being screwed together, is now taken. Its beak should be placed at a more obtuse angle to the shaft than usual in order to facilitate the passage of the instrument through the soft tube. A movable handle having been screwed on to the vesical half of the sound, the surgeon holds the ferrule of the soft tube in his left hand, and passes the sound through the tube till the bulbous extremity of the instrument has fairly emerged from the slit. The tube and the sound are now to be seen *in situ* as when in the bladder. The next step is to remove the soft tube so as to permit of the free manipulation of the sound and the transmission of sensations unimpaired by the coating of soft tube. This is effected by removing the handle and screwing the long stem into the vesical portion. The soft tube can now be retired, and the long stem removed. The handle having been screwed on to the sound again, the instrument is ready for use. Thus by the simple means I have related M. Mercier has enabled surgeons to overcome an otherwise insurmountable difficulty.—*Lancet*, Jan. 25, 1879, p. 115.

68.—ON EXTERNAL URETHROTOMY.

By G. B. MORGAN, Esq., Senior Surgeon to the Sunderland Infirmary.

The external division of the male urethra may either be done as an operation considered to be expedient or it may be imperatively demanded to save life.

In 1844 Mr. Syme communicated to the *Edinburgh Medical Journal* a paper in which he described a form of urethral stricture in which, although a very small instrument may be passed through it, no progress is made in getting it dilated. Constitutional disturbance, with rigors and suppuration, follow the introduction of an instrument, or it is found that after dilatation the dense tissue re-contracts; and the patient, after weeks of treatment, is still suffering as at first. In these cases Mr. Syme proposed to divide the stricture through the perineum, having first introduced a slender grooved staff as a guide upon which to cut. This operation, in suitable cases, and with sedulous care afterwards, gives very satisfactory results, and is not difficult of performance. But sometimes we meet with cases where it is impossible to pass the very smallest instrument, where the urine is distilled drop by drop, either through the urethra or some collateral fistula, and even that only after the most agonising

expulsive efforts, where the swollen face, protruding eyeball, and conjunctiva stained with blood, show how violent and how sustained the effort has been, while during sleep the stinking urine drains from the overdistended bladder, soaking the bed, and making life miserable. Early in these cases, which are often cases of traumatic stricture, extravasation sometimes occurs; but later, when they have become chronic, it is wonderful the amount of hydraulic pressure which the tissues become capable of sustaining.

Now it is in these cases that operative interference is so imperatively demanded, and is at the same time so difficult of performance. If you cannot get a guide into the bladder, you must dissect out the urethra behind the stricture without a guide, and no one who has not himself done the operation can estimate its great difficulty. Sir Henry Thompson says: "In reference to perineal section, or the operation without a guide, the best that can be said of it is that it is a hazardous proceeding. No surgeon should entertain the idea of performing it except after thoroughly assuring himself that the stricture cannot be rendered permeable to instruments, and so made amenable to treatment in some other way. In a few cases the operation is performed successfully, but in most there can be little doubt that the route of the stricture itself is not followed, and that the knife makes a new channel through adjacent and often unsound tissues, very inadequate to perform subsequently the function of a urethra; and in many instances the attempt to make a channel to the bladder has wholly failed, and the patient has been removed from the table unrelieved, and in not a few cases the result has been fatal."

Mr. John Coulson was, I believe, the first to devise the following operation, which is now generally practised. It has been improved upon by Dr. Gouley, of New York, and attention has been drawn to it by Mr. Teevan:—The patient having had the rectum cleared, and the perineum shaved, is to be put into the lithotomy position, and placed under chloroform. A straight staff, with a groove running to within half an inch of the end, and terminating in a button, is passed down to the stricture. The urethra is opened upon this groove, *not upon the point of the staff*. The staff is then made to protrude, and the button hitched in the upper end of the wound, so as to enable the assistant to hold up the urethra by it. The edges of the opened canal are held apart by toothed forceps, and the operator proceeds to search in the cul de sac for the mouth of the stricture, by means of very fine whalebone probes. These should be olive-tipped, but there is difficulty in getting them properly made in England. Having found a passage, a fine silver tube is passed over the whalebone, and if the whalebone has been in

the bladder urine will flow through the tube. The stricture is then divided upwards with a tenotome, directed by the slit in the tube, and then one half of an elastic catheter is passed into the bladder upon the tube, and the other half having been passed through the meatus, the two parts of the instrument are united by a screw-joint, and the patient is removed to bed.

The advantages claimed for this operation are:—1. The greater chance of finding an entrance to the stricture with a fine olive-tipped bougie, applied close to the face of the stricture. 2. The demonstrating that the bougie is really in the bladder, by passing over it a fine silver tube, and drawing off the urine, and so saving the mortification of laying open a false passage. 3. The metal tube serves as a guide for the passage of a catheter over it. 4. Diminishing the risk by dividing the stricture subcutaneously, instead of enlarging the original wound.

No doubt these advantages are great, and the operation should always be attempted in preference to perineal section, but the two cases which I now proceed to narrate show that sometimes it is impossible to find a passage through the stricture, and in one case the patient was saved by perineal section, while in the other success was obtained by utilising a fistulous opening.

Mr. M., aged forty-three, formerly a mariner, a thick-set and strongly-built man. Twenty years ago he fell on board ship and came astride upon a hatchway, the sharp edge striking the perineum with great force. He passed water with great difficulty for some days afterwards, and lost a good deal of blood. The difficulty in making water abated, and he returned to work, but four or five months after he was obliged to enter the *Dreadnought* Hospital with tight stricture. Here an instrument was passed and left in the bladder, but two days afterwards extravasation of urine occurred, requiring free incisions for its relief. Some of these incisions have continued as fistulæ ever since, and the urine has come through them as well as through the proper channel. This latter, however, has become less and less, and four years ago, when he was first seen by me, only a very slight moisture was perceptible at the meatus when he attempted to pass water. His state then—January, 1874—was one of great suffering. Sometimes he would strain for hours, until his face was swollen and livid and the eyes suffused with blood, before a few drops of stinking urine would exude from some of the fistulous openings and give him temporary relief. The perineum was riddled with openings and scarred with cicatrices; on each side of the anus were the marks of deep incisions; on the inner surface of the right thigh were two openings, and three more existed between the anus and scrotum. Marital intercourse had become so painful that it was unbearable. The prostate could be felt much enlarged.

Patient and prolonged attempts were made to pass an instrument through the stricture, but without success; and at this time nothing more was done than administering opium occasionally and directing the frequent use of the hip-bath.

During the next two years and a half he dragged on a very miserable existence, confined to his room and often to his bed, for he found that the slightest cold made all his sufferings worse. On two occasions during this time it was found necessary to tap the bladder through the rectum with an aspirator needle, to relieve his intolerable agony.

In November, 1876, his sufferings had become so great that he became most urgent in his entreaties for something to be done for him; and, having explained to him the great risk of the operation, I consented to attempt his relief.

He was prepared as for lithotomy, and a director passed down to the stricture. Upon this the urethra was opened, and most patient attempts made to find any passage through it. These attempts were continued for more than two hours, so unwilling was I to resort to perineal section—that is, dissecting out the urethra beyond the stricture without a guide; for in his case this operation, always uncertain and difficult, would have been almost impossible, from the seamed and scarred state of the perineum. Having failed to enter the stricture, I determined to try if access to the bladder could not be obtained through any of the fistulæ, and, to my great relief, I was at length able to pass the whalebone guide through an opening to the left of the raphé and along a tortuous passage into the bladder; upon it I passed the silver director, and freely divided the tissues, so that a full-sized instrument could be passed into the bladder. At the time I contemplated a further operation when he had recovered from the prolonged one, but I have found so much reason to be satisfied with what we have gained that I am only too thankful to leave well enough alone. The relief is complete to all the symptoms, and, with the exception of the pain which the passing of some small stones through the new channel has caused, he has had no suffering since the operation. When I felt with the probe that these stones were stuck in the passage, I introduced a sea-tangle tent and left it for a few hours, and when it was withdrawn the stones came with the gush of urine. Coition is no longer painful, the semen coming through the new channel. The character of the urine is no longer bloody and ammoniacal, and he has perfect control over the bladder. Once a fortnight a bougie is passed, as the skin opening has a tendency to close. From the bedridden condition of a confirmed invalid, whose every waking hour was one of suffering, he is restored to his business and to the enjoyment of his life.

Mr. T., aged sixty, a master mariner, came to me on July

16th, 1877, complaining of incontinence of urine, his clothes in the day and his bed in the night being soaked; while he had the greatest difficulty in micturition. His constant straining had produced hernia of the bowel on the left side. He had been a man of somewhat dissipated habits, and stated that about thirty years ago he remembered having an attack of complete retention, which was only relieved with great difficulty by the passage of an instrument on the third day. Since then there had never been complete retention, but year by year the stream had become smaller, and within the last two years the urine had been voided drop by drop, and stillicidium urinæ had become constant. The character of the secretion was ammoniacal and mixed with glairy mucus. The glans penis had several scars upon it, and one cicatrix occupied the site of the meatus, which it had so narrowed that a No. 5 entered with difficulty; beyond this the urethra was narrowed at two points—one about two inches within, and again in front of the membranous portion of the urethra.

He was confined to bed, and placed upon milk diet, with small doses of opium; and day after day attempts were made to penetrate the stricture, but without any success. His condition improved somewhat from the rest and restricted diet, and the stillicidium was greatly lessened as the urine became less acrid; but it was found impossible to enter the bladder. An attempt was made by passing potassa fusa down to the face of the stricture to overcome it; this, however, not only failed, but seemed to increase the difficulty of micturition.

On Aug. 30th he was placed under chloroform, and the operation of Mr. Coulson attempted. He was prepared as for lithotomy, the director passed to the stricture, the urethra opened upon it, and the mouth of the stricture patiently sought for with the whalebone bougies and also with fine silver probes. For two hours on this, the first day, and for a similar time on the day following, these attempts were continued, and were at length with great reluctance abandoned. A day was allowed to intervene, and a third time he was placed under chloroform, and the urethra carefully dissected out, and after some difficulty opened behind the stricture. The latter was then laid open, and the pointed catheter passed into the bladder and out through the meatus. That was on Sept. 2nd. For a few days a catheter was passed daily, then every second day, then once a week, and now (Oct. 31st) the wound is healed, and he passes for himself a flexible bougie, and micturates in a full stream without pain or difficulty. To use his own words, he is a new man, and is comfortable for the first time for twenty years.—*Lancet*, Jan. 18, 1879, p. 78.

69.—ON CATHETERISM IN CASES OF STRICTURE ON PHYSIOLOGICAL PRINCIPLES.

By JOHN GAY, Esq., Senior Surgeon to the Great Northern Hospital.

Cases of stricture, I need hardly say, often come under the care of the surgeon, especially in hospital practice, in which, owing to the patient's neglect, a stricture barely permeable becomes almost suddenly impervious, and the surgeon is called upon to procure a passage of some kind for the urine in the teeth of every obstacle, normal and abnormal, that can waylay his efforts and render them difficult. It is to the earlier period in this (the culminating) stage of such a case that the following remarks are designed to apply.

A man, aged twenty-eight, recently presented himself at the Great Northern Hospital during my visit. He had suffered from stricture for years; had had urethral discharge in abundance, and chronic balanitis as well. Latterly his urine had dribbled away, and, before reaching the hospital, this resource had failed him. Catheterism was attempted by skilled hands, but in vain; and as early relief was necessary, an operation was advised, but refused. On examination, he was found to have a hard, firm, and painful stricture about three inches from the orifice, for which I proceeded to use a catheter on the following principles:—

1. As it is the urethra is absolutely impervious to the passage of the catheter from a combination of causes—viz., the stricture growth engorged with mucus and blood, and rendered painful by futile catheterism; and certainly spasm. It is not, however, absolutely impassable.

2. The tightest part of the stricture is that in front.

3. The unconditional use of a catheter would, in such a state of the parts, certainly intensify the difficulty by calling into play a new source of resistance, in the form of normal muscular antagonism, to its passage—a force that is ever on the alert to oppose the enforced passage of a foreign body through the urethra into the bladder.

4. This automatic force can be brought under complete control by an act of volition, and not only so, but be made to impart to the strictured canal the greatest amount of patency and passivity of which it is capable.

5. The means to this end consist in making the patient bring the sphincters or detrusors of the bladder and urethra into a state of absolute rest by voluntarily, but gradually, calling into powerful action their antagonists, the expulsors or accelerators, and using the catheter whilst the force thus elicited is kept in a state of strain.

6. This mode of palsying the detrusors has another advantage which anæsthesia does not possess, since it assists the surgeon by employing the urine as a dilator, and thus reduces the resistance of the stricture slit.

In the case before us the method thus indicated was carried out as follows:—The patient was made to stand, supported by assistants, upright against a firm support, with outstretched legs—a position I always insist upon in catheterisation, if feasible,—and being prepared with a well-warmed and oiled silver catheter (No. 4, at a venture, in this case), he was called upon to make an effort to pass his water and to gradually increase it to the extent of his power, always under the impressed conviction that he will succeed. After straining thus for a few seconds, and being required to keep up the act until he had permission to relax it, the point of the instrument was gently insinuated into the urethra, and carried on to the stricture. By careful exploration I was soon satisfied that its point and the slight force I was using were in a line with the axis of the canal, and that the entrance of the stricture had been reached. This I *felt*, for I had contrived to slide the instrument along the floor of the passage to the furthest point I could reach in any part of the canal, and by the sense of a slight grip of its point which was given me on making a simple move of the instrument onwards, I was sure that the passage had been gained. The patient still keeping up the strain, with a very little more force the catheter passed through with the usual, not always assuring, jerk. It could not, however, be made to enter the bladder, for its course was interrupted by another stricture at the membranous part of the urethra. This I did not attempt to pass, being satisfied that if the instrument could be retained during the night, the remainder of the passage would be easily passed in the course of the morrow, for the catheter would now indirectly act as an expulsor, and therefore keep in check any renewal of action on the part of any counteracting power. The urine passed abundantly during the succeeding night, not *through* the catheter—for it contained some clotted blood, and if it had not, I should have prevented it by the use of a close-fitting stilette,—but around it; and on my visit the next day, the instrument was passed through with the help of the tip of my forefinger. A severe rigor followed the first effort, which was subdued by a glass of hot brandy-and-water and one scruple of quinine in the course of the next twenty-four hours.

The subsequent treatment has been daily catheterisation, using a larger catheter each day, and allowing it to remain a few hours on each occasion. On the seventh day a No. 8 was

easily passed. I need not refer to the watchful care which is always needed in the after-management of such cases.

I have ventured to ask permission to publish this case, trusting that the principle advocated—viz., that of falling back upon physiological resources as a help in the treatment of severe cases of stricture—might meet with whatever attention it may be thought to deserve.

I may state that I insisted on this method of treating stricture in a paper published in June 1861; and that, although it called forth but little attention at the time, and I believe less since, I have not failed to employ it in every case requiring it, with invariably the like results. I have also demonstrated it in the course of hospital and private practice, so that it is not entirely without its witnesses.—*Lancet*, Nov. 16, 1878, p. 687.

70.—ON TWO CASES OF STRICTURE OF THE URETHRA.

By S. MESSENGER BRADLEY, Esq., Surgeon to the Manchester Royal Infirmary, and Lecturer on Surgery, Owens College.

This is briefly the history of the first case. Mr. D., aged forty-five, had for years suffered from difficult micturition, with occasional attacks of retention, which, until the one that led to my visit, had always yielded to warm baths and a glass of gin and water. The stricture was of long standing, and resulted from a gonorrhœa contracted twenty years before. He had never had an instrument passed into the bladder. On August 1st, 1878, I saw him in consultation with Mr. Smart, who had previously attended him, and who informed me that in spite of patient and persevering attempts to pass a catheter he had failed in introducing anything into the bladder. The retention was complete, of many hours' standing, and the distension extreme. After trying without success to catheterise the patient, I tapped the bladder per rectum and tied in the cannula. This of course gave immediate relief, and also afforded us breathing time for our future plans. With Mr. Smart's permission, the patient was now removed to hospital, and until he left (quite well) he lay in Albert ward. An elastic tube was attached to the cannula, and the urine was thus allowed to escape into a vessel without lying to decompose in the punctured bladder. The confinement to bed, with the use of diaphoretics, having reduced all congestion of the urethra, attempts were now once more made to enter the bladder *per vias naturales*, but for some time in vain. In the meantime his health did not suffer, and he experienced very little inconvenience from the presence of the cannula. After he had been in the infirmary for seven days some urine began to escape by the urethra, and the cannula was removed from the rectum. (It had

been taken out once or twice previously to clean it, but had been re-introduced on each occasion; now it was finally removed.) A singular phenomenon now presented itself; it was found that our patient had unconsciously acquired a perfect sphincter control over the recto-vesical opening. Not a drop of urine dribbled through this aperture; even after a night's sleep the bed remained perfectly dry, but when he wished to micturate then he *sat* upon the vessel and passed water *more femineæ*. I suppose this power to have been due to the puncture passing between some circular figure-of-8 fibres in the trigone of the bladder, presided over by the same nervous guardian as the neck of the bladder. In this condition he continued for about a fortnight, getting up every day unless we wished him to remain in bed for examination purposes, and going about as if nothing ailed him. One day I slipped a ferret into the bladder, and there and then we followed this up in the way with which you are now familiar, until it was easy to pass a No. 4 catheter. In other words, a silver catheter of small size was screwed into the threaded head of the ferret, and readily passed, after Gouley's plan, into the bladder. Dilating tubes were then slid along the catheter in the manner which Mr. Lund introduced and popularised in this hospital, and the whole apparatus withdrawn; then a Holt's divulsor was introduced without difficulty, and the stricture was dilated sufficiently to allow a No. 12 English catheter to enter the bladder without hindrance. Little or no disturbance followed this simple operation, which was almost bloodless, and was, as is always my custom, performed without chloroform; and the patient left the hospital three days later, with instructions to present himself every week for catheterism. This he has continued to do, and as yet the stricture shows no signs of recontraction. I should add that from the moment of dilating the stricture the flow through the rectum entirely ceased, and the opening apparently closed; at all events every drop of urine has, since the latter operation, been voided by the urethra.

Let me now draw your attention to the case of ruptured urethra. During the stay of Mr. D. in hospital, a boy aged nine was brought in with rupture of the urethra from a fall across a railing. Already on admission there was urinary extravasation into the perineum, and the distended bladder rose high into the abdomen above the pubes. Our resident surgical officer, Mr. Southam, drew off the urine with an aspirator introduced above the pubes, and made free incisions into the perineum. When I saw him the next morning, no urine had passed per urethram, but a little had dribbled through the perineal incisions; the bladder was again distended. Having chloroformed the lad, I proceeded to examine the urethra, and found

it apparently completely interrupted in the perineum, of course rendering catheterism impossible. This point being cleared up, I decided upon entering the bladder from the perineum, by the operation introduced, and strongly, though I think not too strongly, advocated by Mr. Cock. Indeed, I am of opinion that in such cases as the one I am bringing before your notice, this is ever the proper plan to pursue. With the left finger introduced into the rectum, I made pressure against the elastic trigonum vesicæ, and then, with a long-bladed finger-knife, cut towards the tip of my finger directly in the middle line, entering the knife at the central tendinous point of the perineum, and carrying it on until the escape of urine informed me I had entered the bladder, or some part of the urinary passage; then, withdrawing the knife, I glided a short silver catheter along the incision into the distended organ, and quickly drew off the urine. I then tied the catheter in, and left him. Next day he was very comfortable, and I resolved to restore, if possible, the natural passage. Accordingly I slid a shallow broad director along the catheter, which I then removed. Next, taking a No. 6 silver catheter, I passed it along the urethra from the meatus till the point emerged in the perineum, when, turning it into the groove of the director, I contrived, after a little manipulation, to slide it along this guide into the bladder. Now the main object was gained; and from that day henceforth my chief anxiety was to prevent, if possible, the formation of a traumatic stricture. With this end in view I kept in the silver catheter, with few intervals, for a fortnight, until the perineal incisions had healed, and afterwards continued to pass an instrument every other day until he left the hospital; then instructions were given to his parents either to bring him to the infirmary every week, or to take him to have the urethra examined by their own medical man from time to time. By this means we hope to save him from the direful consequences of a slowly but surely contracting urethra.

Such are the cases, and now for the lessons they teach.

In the first place, you will notice that in neither case was it possible to enter the bladder by the natural passage, but that we adopted different plans in the two cases; in the first we made a paracentesis vesicæ through the rectum, tying in the cannula; in the second we opened the bladder from the perineum. The reason for acting thus is obvious. In the first the natural passage still existed, and, though we were unable to pass an instrument, we knew that it still existed as a *pervious tube*, for non-traumatic organic stricture never obliterates the passage, and therefore we felt confident that if we could gain time by some simple plan, such as tapping per rectum, we should some day or other succeed in our attempts at catheterism.

As the sequel showed, this was a just confidence, for in a few days we slipped in a filiform bougie; and, having done this, it was, as you know it always is, a perfectly simple plan to there and then dilate the tube to any extent we wished. The second case was different. Here the natural passage no longer existed in its integrity; it was partially or entirely interrupted, ruptured by the fall; hence no such proceeding as tapping suprapubically or rectally would, by giving us time, enable us to restore the urethra. It was therefore desirable to perform such an operation as would enable us to restore the urinary passage, and such an operation we have in the perineal section of Cock.

It, of course, goes without saying that if you can in any case pass a catheter, you will not adopt either one plan or the other; but when you cannot, then, I believe, you will be right in electing, in all impermeable non-traumatic organic strictures, except where there are numerous urinary fistulæ, to tap per rectum, and trust to time and patience, with rest in bed, to enable you to pass by and by an instrument along the contracted tube; and in all cases where the urethra is ruptured by violence, and you cannot catheterise, to perform Cock's operation, and then to restore the passage in the way above described; and, further, this plan of performing perineal section is probably the best to adopt also in cases of impermeable non-traumatic or traumatic strictures, when the perineum is riddled with urinary fistulæ.

And now a word or two before we part upon the subsequent treatment—that is, the treatment of urethral strictures after you have restored the passage sufficiently to pass a No. 4 English catheter. There are three plans open to you—gradual dilatation, immediate dilatation by divulsion, and internal urethrotomy.

In favour of gradual dilatation we can say much; it is perfectly safe, it is not necessarily tedious, and though, like every other plan, it is followed by relapses and reconstrictions, it is probably less liable than the others to this serious drawback. If employed, the best method is to keep the patient entirely in bed, and almost constantly to keep a catheter in his bladder, only taking one out to put another and a larger in its place. In this way a tight stricture may generally be dilated up to admit a No. 12 or 14 English size in a fortnight. Beyond this degree of dilatation I do not care to go, in spite of what the Americans say in favour of much greater dilatation. Personally I do not tell my patients they are cured of a stricture, but, on the contrary, urge them to learn to pass an instrument for themselves or to present themselves for examination three or four times a year. Gradual dilatation practised in the manner now mentioned is certainly calculated to promote the absorption

of exuded lymph and even organised fibrin, but that urethras so treated may yet relapse is sufficiently attested by the history of a case in Mosley ward, in which we followed out this plan *de rigueur*, and yet where the stricture recontracted, so as not to admit a No. 3, in less than three weeks.

In favour of immediate dilatation a good deal may be advanced. When the kidneys are healthy, it is, I think, quite a safe procedure. It is not a very painful one: *e.g.*, I constantly perform it, and never give chloroform; the patient is not confined to bed for more than a day or two, and it is of course speedy. My own experience goes to show that relapses are rather more frequent than from gradual dilatation, but that with proper instructions as to having an instrument passed at stated intervals, and perhaps with a few lessons in catheterism so as to enable the patient to look after himself, this should not lead to future difficulty. In many cases the stricture is confined to the submucous tissue, and this, being naturally more brittle than the elastic mucous lining, will be ruptured by the divulsor without any lesion taking place in the lining membrane; in other cases, where there has been, from ulceration, loss of mucous membrane, of course *both* will be torn. In the former case no hemorrhage takes place; in the latter there is a little bleeding, but always very trifling, and soon ceasing if left alone.

Of internal urethrotomy I have no personal knowledge; but, although I am aware that abler and more experienced surgeons than myself practise and speak highly of it, I cannot but regard the operation with extreme suspicion and disfavour. In boldly criticising it, then, as dangerous and unsatisfactory, I would have you remember that it is the criticism of one who has never performed it, and who, I think I may add, never will. I dislike the operation for the following reasons: it is perilous at the time, for I have known death to follow from hemorrhage in a few hours; it is pregnant with danger for a considerable time after the immediate risk is over, for I have known death from septicæmia to follow ten days after its performance; and it is not attended with any advantages over immediate, and still less over gradual, dilatation, in being less prone to relapses, for I have known a case twice operated on by internal urethrotomy which yet resisted the passage of any instrument a few months after the second operation. I quite fail, then, to see what there is to commend this elegant operation. It is true we hear of "organisation of the blood-clot," and of the "cicatricial splice" thus fashioned being more elastic than ordinary scar tissue. Is not this nonsense? Imagine the crowning glory of antiseptic surgery—the organisation of the blood-clot—taking place in an incised urethra; that is to say, in an incision washed every few

hours in urine! The greater elasticity of the cicatricial splice, again, however it be formed, is mere matter of assertion; there is nothing to show *why* this should be so, and there are no reliable comparative statistics to prove that it *is*. In those cases of tight, or it may be impermeable stricture, where the perineum is riddled with fistulæ, the remedy is not, as it appears to me, internal but external urethrotomy; and I myself anticipate the day when internal urethrotomy shall be relegated to the limbo of discarded surgical operations, and when we shall be content to treat strictures of the urethra without this modern improvement.—*Lancet*, November 30, 1878, p. 759.

71.—CONCERNING THE OPERATION OF JOHN HUNTER IN CERTAIN CASES OF IMPASSABLE STRICTURE.

By Dr. J. A. MACDOUGALL, Surgeon to the Cumberland Infirmary.

At a late meeting of the Clinical Society of London, Mr. Callender brought under the notice of its members some cases of impassable stricture in which Hunter's operation had been performed, and one of them which was under my care I have thought of sufficient interest thus to record.

W. T., æt. 47, a railway labourer, was, early in last May, admitted into the Cumberland Infirmary, suffering from stricture of the urethra and perineal fistula. His story was this:—Fifteen years ago he first suffered from symptoms of stricture, the result of a troublesome gonorrhœa. Five years later he sustained, by an accident, severe bruising of the perineum, and from that time he had much and increasing difficulty in making water. Ultimately his symptoms became so urgent that he was admitted to the hospital at Portsmouth the subject of complete retention.

Relief was at first obtained by the warm bath and by opium, for it was some days before an instrument could be introduced. Finally, perineal section was performed, and he left the hospital able to pass for himself a medium-sized catheter.

This he perseveringly continued to do until nearly a year ago, when it was stolen from him. From the date of its loss his old symptoms began to re-appear, and in a short time his difficulties were well-nigh as great as ever. While thus suffering he was much exposed to cold and wet, and his work was very laborious. Suddenly there came severe pain and swelling in his perineum, attended with almost complete retention, and this continued for several days. Then the swelling gave way externally, the urine escaped by more than one opening, and his relief was speedy and great. From time to time, however, fresh collections of matter formed, followed by fresh fistulæ, every one of

which gave exit to urine, so that at last the quantity passed by the meatus passed guttatim. For five months, however, before his admission even this small quantity had stopped; *he had passed absolutely none by the penile urethra.*

On examination it was found that the perineum, the scrotum, the penis, and the textures over the pubes, nearly midway to the umbilicus, were dense, indurated, and largely œdematous. There existed in all nine fistulæ—four in the perineum, three in the upper and inner surface of the left thigh, and two above the pubes. The surroundings of all of them were more or less inflamed-looking, and from several there was purulent discharge. On proceeding to pass a catheter, so great was the infiltration of the penis that it was with considerable trouble the meatus could be found. Once entered, the instrument passed easily to within an inch of the root of the penis, where it was arrested. A No. 2 could with difficulty be got beyond this, and passed as far as the membranous portion, then it entered a narrow passage lying immediately to the left of the centre line, and along this it could be carried until its point was felt per rectum. There, however, it had deviated so much more that there could be no doubt it had entered a false passage. Blocking this passage with a filiform bougie, I made, by introducing another, a most careful examination of the ordinary urethral line, but quite failed to find a way. This manœuvre was tried upon more than one occasion, and in the hands of both Mr. Page, our consulting surgeon, and myself, small catheters and the fine steel instruments had fair and persistent trials, but always in vain.

The man's general condition was, moreover, unfavourable. He had been for many years a hard drinker; his heart's sounds were feeble and indistinct, his liver was markedly enlarged, and his urine was not only largely albuminous (nearly one-fourth), but microscopic examination showed the presence of waxy and fatty casts in considerable numbers.

Rest in bed, the occasional use of the warm bath, and a purely milk diet not only did his general state good, but it somewhat improved the local condition. The albumen in his urine diminished very considerably, the œdema became somewhat less, the inflammatory irritation subsided, and he made less complaint of the distressing scalding.

Still his discomforts and his suffering were great, and he was most anxious that something should be done for his relief.

After careful consideration I determined (and the grounds for this determination I give hereafter) that the operation of John Hunter promised most success. One great difficulty lay in the fact that the man had no command over his bladder when it contained above an ounce of urine. Then it simply flowed

from him. With a view to overcome this, he had from time to time belladonna given as suppository, and he was instructed to exercise all the retaining power he could command. Thus he improved, for *gradually* the intervals lengthened, until on the morning of his operation he was able to retain water for fully *three* hours.

20th June.—Chloroform was given him, and an incision about three inches long, carried well *over* the pubes, was made in the linea alba. The textures were carefully divided until the upper surface of the pubic bone was bare and distinct. The aponeurosis between the recti was opened; the origin of the pyramidales divided by lateral incision, and the parts pulled apart. Reaching the peritoneum, and pushing it carefully back, the dissection was continued close behind the bone until the level of the bladder was reached. So contracted was it that its recognition was not easy; but for this difficulty I was prepared. Attaching a fine needle to an aspirator I thrust it into the viscus, which from its position seemed most likely to be the bladder, and at once withdrew fluid. This, upon boiling, not only became very speedily cloudy, but by its smell fully determined its character. Retaining the needle as a guide, there was no difficulty in making with a scalpel such an opening as admitted a No. 9 gum-elastic catheter. The parts were then carefully sponged with a strong carbolic solution, and as there was likely to be a certain flow of urine by the side of the catheter, as well as serum from the exposed surfaces, I introduced behind the pubes, in front of the bladder, a large sized glass drainage tube. Through this, by means of a pipette, considerable quantities of fluid were withdrawn during the first forty-eight hours after operation. A single deep stitch was placed in the upper part of the wound; an india-rubber tube was attached to the catheter, and carbolic dressing was applied. The india-rubber tube was ultimately arranged so as to act as a syphon, and during the eight or nine days the patient survived, it acted steadily and satisfactorily. Twenty-four hours after operation he had a most acute attack of bronchitis. From this, however, he rallied, and on the fourth day—spite some trouble that had occurred from the catheter slipping out of his bladder—he was hearty and well.

Already *four* of the fistulas had apparently closed. The œdema had disappeared from the pubes, and the penis and scrotum were much less implicated. He expressed himself as much relieved and more comfortable. Another four days passed satisfactorily; the local condition was immensely better; the wound was well-nigh healed; his temperature was 99°, his pulse normal, when, without warning, he was seized with most violent hæmatemesis. This continued more or less for some hours, and he sank exhausted on the ninth day.

A sectio revealed, near the pylorus and situated in the great curvature of the stomach, a large ulcer, in the bottom of which was the open mouth of a large artery. The viscus itself and the small bowel contained a considerable amount of blood. The heart and liver were large, one kidney was contracted and cirrhotic, the other was fatty. The bladder was very small, with much thickened walls, but there was no *cystitis*, and there was no trace of inflammatory irritation in the peritoneum or in the tissues of the pelvis.

A medium-sized catheter could now be passed through the stricture situated *anterior to the scrotum*, but when pushed steadily on in the urethral line it passed through the upper wall of the canal near to the bulb, and lay on a considerably higher level than the membranous portion. That portion was in fact lost in the midst of a dense cartilaginous mass, a tubular network alone marking its old line.

The life history and the postmortem dissection in the above case demonstrate it to have been truly one of impassable stricture. Comparatively rare such cases are—their rarity, I doubt not, largely due to the admirable teaching of the late Mr. Syme. In dealing with them several methods present themselves, and as mayhap pointing the best lesson from the case narrated, I will refer shortly to those which came under consideration in connexion with it, and show my reasons for choosing the procedure I followed.

This much, however, I would premise, that the man's condition demanded imperatively that the operation undertaken was one likely to be followed by *immediate* benefit. Moreover, a *sine quâ non* was, that the urine should find such a channel as would quickly divert it from the multiple fistulæ.

The method of Wheelhouse, from its known efficacy in many apparently impassable strictures, suggested itself; but the existence of a tight contraction in the *penile* urethra, and the decided belief that the canal in great part of its membranous portion was obliterated, negatived its performance. The likelihood of Cock's operation proving beneficial was very carefully considered; but the existence of a large fistula (the largest of all) about an inch in front of the anus, just in the line of the apex of the prostate, and the existence of which had seemingly much to do with his distressing incontinence, gave rise to some doubt whether a *larger* opening in this situation would do much good.

The perineum, too, was dense and deep, and had the urethra been opened at this point it would, I imagine, have been very difficult to pass an instrument in a forward direction through it. Suppuration, moreover, had long gone on in the neighbourhood, and the anatomical relation of the apex might have been altered.

Such considerations made me uncertain as to its ultimate success—nay, even its immediate benefits, and it was passed over. Opening the urethra per rectum, as practised by Mr. Furneaux Jordan, also claimed notice, but the suspected state of the urethra was against it. To succeed—and in certain cases it seems a most admirable and successful operation—the continuity of the urethra must be *speedily* restored, so as to allow the wound in the rectum to close. In the foregoing case this could not easily have been done, the existence of the anterior stricture offering an all but insuperable obstacle to the performance of “external urethrotomy,” *i.e.*, the operation performed *without* a guide.

Failing all these, there remained the operation proposed by John Hunter. I was aware that this had been successfully performed by Mr. Callender, in the first instance, in just such a case as it was originally meant to meet, where a suprapubic opening already existing, instruments could be passed upon the stricture from behind. He had, however, also dealt favourably with a case similar to my own, making by dissection the puncture of the bladder, and by means of it ultimately restoring the urethra. It was the issue of this case that largely determined my practice in that of W. T., and the comfort and relief it brought even in its early steps augured well for a good result. Had he not suffered the untoward accident of internal hemorrhage—and, strangely enough, he never had previously *a symptom of gastric ulcer*—I believe he would have done well. That the urethra could have been restored I also believe, and even had we failed—and this doubt weighed with me greatly in selecting the operation—he would by means of a plugged canula have had ultimately much more command of his bladder than he possessed when the operation was done. The short time he survived served to show how much the local condition was benefited. The œdema was rapidly disappearing; the fistulæ were closing, and the irritable, raw condition of the skin, the result of the constant contact of urine, had disappeared. The anterior stricture, admitting previously with difficulty a No. 2 catheter, now allowed No. 6 to pass, thus securing a thoroughly dependable guide to the point at which the urethra was lost. Mr. Howse of Guy's has, adopting this operation, treated by means of it a case which could have been managed, I believe, by no other method, and has demonstrated that it has a greater and wider *rolê* than Hunter meant it to play.

The cases in which it is necessitated are assuredly most rare; still they will occur, and the possession of this way of meeting them adds somewhat to the respect and gratitude we owe John Hunter.—*Edinburgh Med. Journal*, April 1879, p. 873.

72.—SECTION OF THE CORNEA IN HYPOPYON KERATITIS.

By Dr. A. EMRYS-JONES, Resident Surgeon, Manchester
Royal Eye Hospital.

Considerable discussion has recently taken place as to the value of the above method of treatment, introduced into ophthalmic surgery by Dr. Saemisch in 1871. During the last two years I have taken especial interest in the subject, and have carefully recorded notes of thirty-five cases which occurred in the practice of this hospital, to the medical officers of which I am greatly indebted for valuable assistance in the inquiry; and I now propose to mention briefly the results, as well as the practical remarks that have suggested themselves to my mind during the course of treatment. Saemisch gives this form of traumatic ulcer the name of *Ulcus corneæ serpens*. The history the patient generally gives is, that he has been struck in the eye with a piece of iron chipping or some other foreign body, and has suffered intense pain, from which he can get no relief. On examination, a small ulcerated cavity, filled with suppurative débris, is found at one spot, with irregular and extending margins and surrounded by a halo of diffuse cornea; a quantity of pus is found in the anterior chamber, which sometimes increases very rapidly, but which at other times becomes effused very slowly. The conjunctiva is red and inflamed; and there is generally iritis, and intense pain due to pressure on the ciliary nerves.

Treatment.—Any foreign body impacted in the cornea or elsewhere should be looked for and removed without delay. The pupil should then be dilated, if possible; and for this purpose we use a strong preparation of atropine with glycerine (introduced into the eye with a camel's-hair brush frequently), and for which I subjoin the formula, as it will be found most useful in breaking up commencing adhesions:—Sulphate of atropia, thirty-two grains; powdered starch, twenty grains; glycerine, one ounce. (One drachm of the mixture contains four grains of atropia.) We should endeavour to allay the pain by the administration of morphia by the mouth or (better) hypodermically, and the application of hot chamomile and poppy-head fomentations.

Should we succeed in dilating the pupil and relieving the pain, the frequent instillation of atropine drops (two or four grains to an ounce) and application of hot fomentations will suffice. It is important in all these cases to give good food and a moderate amount of stimulants, preferably beer. In a large number of cases, however, this treatment will be found

to give no relief, and to prove no check on the destructive ulcerative process which threatens rapidly to destroy the eye; or the condition may be far advanced when it is represented first to our notice—poultices have been applied, numberless lotions have been tried, and often blisters and leeches have been employed, to render the already pitiable patient more miserable still. In either case, where atropine and fomentations fail, or where the case is already far advanced, bolder measures must be promptly resorted to. The best plan, according to our experience, is to make a *bold incision* through the lower third of the cornea, extending through its whole breadth, exactly similar to the incision generally made in the modified Graefian extraction of cataract. Saemisch advocated a *limited* incision through the ulcer from the sound tissue on the one side to similar tissue on the other side; but our cases prove that this is not essential, or even desirable, unless the ulcer is situated on the lower third. The broad incision most effectually relieves the existing tension, which is one of the most active factors in the causation and propagation of suppuration. Having made the incision, the curdy pus will generally escape without any further manipulation, or with a very little coaxing with a vulcanite spoon; if not, it must be removed with a pair of Graefe's straight iris forceps. The patient instantly expresses great relief from pain. He should be sent to bed and kept perfectly quiet in the recumbent posture for a few days. We should watch daily for any signs of reaccumulation of pus, and if any be found, the margins of the incision should be at once separated with a fine No. 1 Weber's lachrymal probe or the point of a Weber's knife.

Out of the thirty-five cases, it has been only found necessary to reopen the wound once in four cases, twice in only one case, thrice in only one case, and never oftener. In judging the result we must, of course, take the condition of the eye on admission fully into consideration. In twelve of our cases the anterior chamber was more than half full of pus, in six the greatest part of the cornea was in a suppurative condition, in four there was the additional complication of a large lead opacity. In spite of the aggravated and advanced condition of many of the cases, in twenty-four there resulted very good vision, varying from No. 1 to No. 10 Jäger's test-types; in six fair vision, Nos. 14 to 20 Jäger; in four good perception, and in one partial staphyloma (due to neglect of patient). In six cases iridectomy was performed some months later, owing to resulting permanent opacities.

Remarks.—From careful analysis of the above cases, the following advantages may be claimed for a bold section:—1. It gives instant relief from pain. 2. It provides effectual relief to

the pus. 3. It proves an immediate check to the ulcerative process. 4. It is the means of restoring useful vision in apparently hopeless cases. 5. It gives rise to little or no deformity, and is followed by no ill consequences. 6. Iridectomy can be performed with greater advantages afterwards, if required, from resulting opacities.—*Lancet*, Jan. 18, 1879, p. 81.

73.—ON THE EXTRACTION OF FOREIGN BODIES FROM THE EAR.

By Dr. J. MARION SIMS.

The first article I ever wrote for a medical journal was on this subject, and, as appropriate now, I quote from it the following:—

‘Whoever follows the loose direction of ‘syringing the ear forcibly,’ will often be disappointed. A sufficient explanation of this may be found in the anatomical peculiarities of the organ. The course of the meatus is curved, running at first forwards and upwards; then downwards and backwards; its concavity looking obliquely downwards and forwards. It is an inch and a quarter or more in length; about a quarter of an inch in its transverse diameter, a little more in the perpendicular direction, its section being oval. Its dimensions vary, being a little contracted at the orifice, still more so in the centre, which is the narrowest part, and again at the tympanum. The anterior semilunar edge of the concha, leading into the meatus, projects considerably below a line forming a tangent with the convexity of the lower wall of the canal. With the obliquity of this canal and its irregular dimensions, it will be found difficult, if not quite impossible, to throw a stream of water with any degree of force against the tympanum, even when the organ is unobstructed; for, if the nozzle of the syringe be directed parallel with the tragus, the force of the injection will be spent against the anterior inferior wall, or convexity of the floor of the canal; if turned more towards the semilunar ridge of the meatus, its power will be lost on the posterior superior wall, or convexity of this tube. But, let us change the direction of the canal by laying hold of the lower portion of the antihelix, pressing the dorsum of the concha forwards while we put the whole organ forcibly on the stretch, pulling it outwards, upwards, and backwards, thus making the meatus a straight channel of diameter, and then it will be found comparatively easy to throw a uniform stream of water to its very bottom. If the foreign body be small, the water will dislodge it as it rebounds from the tympanum; if it be large, plugging up the passage perfectly, two or three injections properly directed (the ear held as described)

will be certain to change its relative position sufficiently to allow the next to pass by, impinge on the membrana tympani, and wash it out in its retrograde course, or so far dislodge it as to bring it within reach of the forceps. The syringe that I use holds about two drachms of water. The nozzle is of small diameter, and the piston is perfectly air-tight. Water, thrown from it suddenly and forcibly, does not pass in a long slow continuous stream, but seems to strike, as it were, all at once and with great power. A syringe of small dimensions is preferable to a large one; because, with a small one having a small tube, the stream may often pass undivided by the foreign substance, so that its reflex action is almost as strong as its direct force. On the contrary, in a large syringe with a large tube, throwing a larger volume of water, a small portion only of the stream may pass behind the foreign body, while a larger portion strikes against it in a direct line with greater momentum, thus counteracting the retrograde force."

The principles here laid down are as true to-day as they were a third of a century ago, when they were first penned. So universally successful have my efforts been in removing foreign bodies from the ear, that I was surprised to find that the same unfailing success did not attend the efforts of all other surgeons. And so I felt anxious to ascertain the reason of this. To this end, I went to three of the most renowned instrument-makers in London, and asked to see the best ear-syringes they had; and they showed me large clumsy metal syringes, with large nozzles, throwing a stream of water large enough for a rectum syringe. I then went to some of the best instrument-makers in Paris, and I found just the same sort of ear-syringes as I found in London. It was then plain to my mind that the fault was not in the principle, but in the clumsy and unsuitable instruments used.

The ear-syringe that we use in my country is made of hard india-rubber, holds only about four drachms, is light, and worked with one hand, while the other is used to straighten the canal by pulling the ear outwards, as above described. But, as said before, the syringes that I find here are of metal, large and unwieldy, with large calibre, throwing a large stream of water, and worked with difficulty.

The American india-rubber ear-syringe is two and a half inches long, the same in circumference, and holds about half an ounce. The orifice of the nozzle is one *millimètre* in diameter. A larger ear-syringe, one holding an ounce, is made by the American Rubber Companies; but I prefer the small one, as it throws a smaller stream of water.

The easy removal of foreign bodies from the ear depends upon two things that are indispensable.

1. A light small syringe, with a small nozzle, throwing a small jet of water suddenly and forcibly, must be used.

2. The auditory canal must be made straight, by traction outward of the lower lobe of the ear, at the moment of injecting water into it.

With these simple directions, failure is impossible.—*British Medical Journal*, Dec. 14, 1878, p. 868.

DISEASES OF THE SKIN,

74.—OLEATE OF ZINC IN THE TREATMENT OF ECZEMA.

By Dr. H. RADCLIFFE CROCKER, Assistant Medical Officer in the Skin Department, University College Hospital.

To the several valuable preparations of the oleates for which we are indebted to Mr. Marshall, the oleate of zinc promises to be a most useful addition. I now wish to draw attention to its utility in eczema. Mr. Martindale of New Cavendish Street prepares it by stirring together one part by weight of oxide of zinc with eight fluid-ounces of oleic acid as free from palmitic acid as possible, and, after letting it stand for two hours, heating it until the zinc is completely dissolved. On cooling, it forms a yellowish-white hard mass, which can be made into the consistence of ointment by the addition of one part of vaseline or olive-oil or two parts of lard or simple ointment. Vaseline is preferable, as it is not liable to change. The other preparations, soon becoming rancid, should be freshly made, and then answer equally well and are much more economical. Mr. Martindale has also spread it on thin paper, which is a convenient way of applying in some cases.

The above preparations I have found very effectual in acute and chronic eczema in the discharging stage. In the dry stage, it is also useful; but in many cases more stimulant remedies cure more speedily. The two following will show its mode of application and its merits, and are chosen as being the first two in-patients on whom it was tried, while the extent and duration of the disease were sufficient tests of its value.

Case 1.—O. N., a boy aged 7, was admitted into University College Hospital on May 7th, under Dr. Tilbury Fox, with a history of having been treated in a provincial hospital for twelve months for general eczema with only slight benefit. How long he had been suffering before this was not ascertained. On admission, the face, neck, and scalp were covered with pustular eczema. Thick yellowish crusts, with copious purulent discharge, were present in these situations; and the eyes were nearly closed by discharge and swelling. Both buttocks, thighs, and legs were also affected; and there were a few patches

on the trunk. The discharge was free, but not purulent in these parts. The following treatment was employed. The hair was cut off, and the parts soaked in olive-oil all night to soften the crusts, which were then bathed off with warm water. On May 8th, the oleate of zinc ointment, made with lard, was spread upon linen rags and applied closely by means of a bandage; the parts were kept constantly moist with the ointment; new crusts were removed as fast as they formed; and every day all the old ointment was wiped off before fresh was put on. A drachm and a half of "Parrish's food" and a drachm of cod-liver oil twice a day were also ordered.

May 13th. Great improvement was manifested. The discharge and scabbing were much less. He continued to improve; and on May 27th it was noted that the face, scalp, and neck were now almost clean, with only slight discharge; while the buttocks and legs were quite dry. On June 2nd, there was no discharge anywhere, and only patches of diseased surface were left, with thin dry scales; and thymol (five grains to the ounce of ointment) was then added. On June 13th, he was discharged well, with no trace of eczema anywhere, and the general health much improved.

Case 2.—A boy, aged 10, had chronic eczema on the thighs and legs for two years. The medical man who sent him wrote: "He has been to all the surgeons in the neighbourhood, and no one appears to do him any good." He was admitted on May 23rd. Similar treatment to that of the first case was employed, and he also was discharged cured on June 13th.

In addition to the above, I have treated more than twenty out-patients during the last six weeks with this remedy. Six have been quite cured, and the rest are still under treatment; but all are improving—some very rapidly—in spite of the disadvantages attending out-patient treatment, owing to imperfect removal of crusts, the intermittent application of the dressings, &c.

The oleate of zinc ointment is a remedy of the same class as Hebra's unguentum diachyli; and, while beneficial in all forms of eczema, its most striking effects are seen in the discharging stage, and, so far as my experience has gone, it never seems to do harm in any case, as happens when stimulating remedies are injudiciously applied.

Since writing the above, three months ago, I have treated a large number of cases with this remedy, with most satisfactory results, so that I can with confidence recommend it as one of the most useful preparations for eczema that we possess.—*British Medical Journal*, Oct. 26, 1878, p. 622.

MIDWIFERY,

AND THE DISEASES OF WOMEN AND CHILDREN.

75.—REMARKS ON THE USE AND VALUE OF THE LONG FORCEPS.

By Dr. THOMAS RADFORD, Manchester.

In my first case, which happened in the year 1817, and for several years afterwards, I used the late Dr. Haughton's long forceps, which have blades of equal length and long handles; the shanks run off in a bow from the shanks. To remedy this last defect, I had an instrument made with parallel shanks, but I omitted making any other alteration. With both these instruments, the infants were sometimes dead, and in most cases there was evidence of different kinds and of different degrees of mischief, which had been inflicted by the injurious compression of badly constructed instruments. Since I altered the construction of my forceps, I have always had great confidence in a judicious use of the instrument; and I have invariably stated that, when it was used in proper cases, the long forceps may be truly considered as compatible with the lives of both mother and her infant. According to the result of my practice, I feel warranted in saying that, if the infant be vigorously alive, the case in every way appropriate, the operator judicious, and if the instrument be well and rightly constructed, the infant ought to be saved.

The head of the infant is, before birth, so wisely formed and adapted as to safely bear a considerable degree of pressure without injury; but although this is the case, yet it must be understood that there is a limit beyond which it cannot be endured without proving destructive to the life of the infant. I feel convinced that the pressure produced by the labour-pains can be longer and more safely endured than it can under artificial compression by the forceps. If, then, the pressure produced by the forceps be really so destructive to infantile life, it must be proper to adopt effective means to obviate this mischief. The handles of midwifery forceps in general are too long, and thereby afford the operator power to squeeze the head of the infant to such an extent as to be certainly destructive of its life. The handles of the forceps ought to be so short that undue

pressure cannot possibly be made. In my opinion this instrument ought to be chiefly used as a tractor, and only limitedly as a compressing power.

To save the life of the infant by the use of the long forceps is doubtless the object of every obstetrician; for unless this were his intention, it would be better at once to have recourse to craniotomy.

To insure the successful application of the long forceps in saving infantile life, the practitioner should keep constantly before his mind the dangers of protraction, and recollect that these dangers increase in a ratio proportionate to the length of time the labour is prolonged.

It is, then, the duty of the obstetrician to ascertain the real condition of the labour, and to become aware whether there exists any cause of obstruction in the brim or cavity of the pelvis, which may require artificial aid to bring the head of the infant forwards. If this condition be known before the membranes are ruptured, so much the better; but if the practitioner be not aware of the existence of an obstructing cause before the waters are discharged, no time should be lost in thoroughly acquainting himself with it. If the pelvic brim is so diminished in its measurements as to require the use of the long forceps, the sooner they are applied so much the better, before the pelvic structures have been subjected to such a degree of pressure as will assuredly produce a further diminution of space. In such cases, it is unwise to wait for the full dilatation of the os uteri before the forceps are applied after the rupture of the membranes, as it is impossible for the head to be forced upon this aperture so as to dilate it.

I will now make a few observations on the practice of introducing the forceps before the os uteri is fully dilated. It is reported (*Journal*, p. 284): "Dr. Swayne, in reply, said that much was owed to the Dublin School of Midwifery in teaching us to use the forceps before the os uteri is fully dilated."

If the opinions of many of the leading accoucheurs of Dublin were examined, it will be found that this practice is decidedly disapproved of by many. (See Discussion at the Obstetrical Society on the Report and Clinical Records of the Rotunda Hospital, *Dublin Jour. of Med. Science*, August 1878, p. 140.)

This practice, if received, as an unconditional rule in forceps operations, would be most dangerous and fraught with the mischief of organic lacerations. It is a practice which should be considered as only conditional. The forceps may be safely introduced by an experienced obstetrician, such as the Master of the Dublin Rotunda Hospital; but it is not a safe procedure to be accepted as an unconditional operation and to be generally adopted.

The proposition to use the forceps before the os uteri is fully dilated, did not originate in the Dublin School, but I had adopted and recommended the practice before I delivered my address at the meeting of the Provincial (now British) Association at Manchester, September 1854. (See my *Observations on the Cæsarean Section and other Obstetric Operations*, page 30.)

I say: "To wait for the dilatation of the os uteri after the rupture of the membranes is a great mistake; for, in the great majority of cases which require the aid of the long forceps, this organic change cannot take place. The obstacle being at the brim of the pelvis, the head of the infant cannot be pressed down upon it, so that before this change happens irretrievable mischief may be inflicted by the continued pressure which the pelvic tissues must under such circumstances endure."

For a description of my long forceps, see my *Essays on various subjects connected with midwifery*.—*Brit. Medical Journal*, October 26, 1878, p. 622.

76.—A DESCRIPTION OF TARNIER'S FORCEPS.

By Dr. ROBERT BELL, Physician to the Glasgow Institution for Diseases of Women.

It is the most perfect instrument, 1st, because we can procure delivery by its means with a much less expenditure of force than by the ordinary instrument; 2d, because we can ascertain in what direction the force ought to be applied; and 3rd, and most important, because it allows the head to rotate freely in its progress through the pelvis, and thus allows of the head accommodating itself to the peculiarities of the pelvis, and in this way imitating nature in each individual case. But while I hold it is the most scientific instrument of the kind we possess, it appears to me quite capable of undergoing many improvements in its manufacture before it can be called the *beau idéal* of what such an instrument ought to be. At the same time, my opinion is that it will be most difficult to improve upon the principle involved in its construction. It must be admitted, however, that the manufacturer, with a little pains, could produce a much less formidable looking apparatus, and a much more elegant piece of workmanship without in any way interfering with its strength (a quality, by the way, which all midwifery instruments should never lack). I must confess, when the forceps first came under my own observation, the impression they made upon me was anything but a favourable one; indeed, I determined there and then that I would not even purchase them as a curiosity—they appeared to me so very formidable and unwieldy. This, however, was only my state of mind before I had seen them applied, and had obtained a

correct knowledge of their simplicity and eminent adaptability to the purposes for which they are intended. M. Tarnier very kindly met me at his hospital in Paris, and not only demonstrated to me the simplicity of his forceps, but the wonderful facility with which they can be applied, and the great advantages they possess over every other kind of midwifery forceps. But, before endeavouring to demonstrate the various merits which the instruments of Tarnier most undoubtedly can lay claim to, with your permission I will make one or two remarks on the forceps in ordinary use.

It must be confessed that the long forceps in ordinary use are not the most satisfactory instrument that could be wished; indeed, the very reverse is the case, and it does not require a very long acquaintance with them to convince one of that fact. If we look at the anatomical construction of the parts concerned in labour, when we remember that the axis of the inlet of the pelvis is represented by a line drawn from the centre of the plane of the inlet downwards and backwards through the middle of the cervix, we cannot but perceive how inapplicable the long straight forceps are when the head is obstructed at the brim, and how dangerous even the long curved forceps are. Let us look for a moment at the long straight forceps, and we see that it is impossible to apply them without pushing back the perineum to a most dangerous extent, and even when one has been successful in getting a hold of the child's head the catch is very insecure, and there is, therefore, very terrible danger of the blades slipping and of the horrible consequences of such an accident. Of course, when it is possible to deliver without any such untoward results arising, the force employed in bringing down the head must be very much less than when a curved instrument is used, because we exert the force in the direction of the axis of the inlet, but then, as we all know, it is next to an impossibility to get a sufficiently secure hold when the head is at the brim with the long straight forceps, so that practically they are useless in such circumstances. We must then have recourse to forceps which have more or less of a curve. By this means we certainly obtain a much more secure hold of the head, but then there is the drawback that we are unable to pull in the direction of the axis of the inlet, so that it is requisite to bring more force into play to procure the same results; and as this extra force is brought to bear, in fact is expended, on the head of the child and soft parts of the mother, and that to their hurt, it is easy to understand how desirable it would be to have an instrument which would do away with this unnecessary expenditure of force and its baneful consequences. If the curve is slight, then, of course, the traction power will be proportion-

ately less than if the curve were greater; but then, if the curve is slight, there is the danger of the forceps slipping, besides the greater difficulty in applying them, and the enormous danger to the perineum. If the curve is greater, there is certainly procured a firmer hold of the head, but then consider how much more force it is necessary to spend in procuring delivery; indeed, the curve in many instruments is so great that a much larger amount of force is actually employed in doing mischief to the soft parts than is used to effect delivery. What mischief is done to the bladder in such cases no one can estimate, and how many cases of vesico-vaginal fistula have their origin in such circumstances no one ever will know. The fact of the matter is that long forceps always do mischief. If the curve is slight the perineum is in danger; if more decided then the bladder is in jeopardy. But these are not the only drawbacks of the ordinary long forceps. They do not permit of rotation of the head, or allow it to accommodate itself to the peculiarities in the shape of the pelvis. One is certainly recommended to "humour" the forceps, and thus allow the head to glide through the passage in the easiest manner possible; but when a man is engaged with a stiff forceps case, when he is undergoing a course, perhaps a brief one, of most profuse diaphoresis, when he is in abject horror in case he is going to be baffled, and when he is, from a faulty construction of the instrument, actually *compelled* to use double the amount of force that is really necessary to deliver the child, I conclude that he is not in the humour to "humour" the forceps.

Then, again, if the blades of the instrument have a curve sufficient to enable one to get a secure hold of the head at the brim, they are entirely useless as a means of rotating the head, nor are they able to rotate with it in its progress through the parts. If rotation were insisted on, then the consequence would be a ploughing up of the perineum by the blades. So much for the ordinary long forceps. Let us now look at the instruments of Tarnier, and I need hardly say that they have received many modifications since Tarnier first introduced them, but I have the pleasure of showing to you the most recent model. What first suggested the idea of the instruments was the employment of the traction power in the ordinary instruments by means of tape passed through the fenestra, this being used as the tractor, while the handles served as compressors. By this means traction is brought to bear on the blades without the intervening powerful lever which exists between the blades and the handles when the latter are used as tractors. In this way, of course, the pull is direct upon the child's head, and the handles merely act as indicators of the course in which the child's head is travelling, while it has every freedom to follow

whatever course is most easy for it to traverse, freedom to rotate being thus given to it. This, then, is the grand principle of Tarnier's forceps. The handles serve to place the blades in position, and afterwards act as indicators of the axis in which the head is moving, while all the force that is necessary to aid delivery is brought to bear on the child's head directly and solely. It is allowed, nay, encouraged, to take the easiest course it can pursue, and we can always tell in what direction to pull by the position which the handles occupy. Force is never wasted on the soft parts of the mother, and the fatigue endured when Tarnier's forceps are used is nothing compared to that one undergoes when the ordinary long forceps are employed.—*Edinburgh Medical Journal*, April, 1879, p. 890.

77.—A NEW TRACTOR FOR OBSTETRIC FORCEPS.

By Dr. ALEXANDER DUKE, Assistant Physician to the Rotundo Hospital, Dublin.

Anyone who has had a fair amount of experience in forceps cases must have often felt the want of additional tractile power, especially where the head was more tightly wedged than usual in the pelvis, or when, from misproportion, it required more than the ordinary amount of exertion with the strength at the operator's command to effect delivery. Having seen more than once an experienced accoucheur fail with all his force to deliver till at length by another grasping him round the waist, he was enabled to do so, I had often considered if some means could not be devised which in a difficult case would give the required power without the necessity of calling an assistant (even if one were always at hand). I was, therefore, much pleased by reading in a number of the *British Medical Journal*, for June 29th, 1878, a description of tractors invented by Mr. Morgan, of Lichfield, to be hooked into the fenestra of any long forceps *after introduction* (in imitation of a more portable and cheaper form of the principle of M. Tarnier's instrument), and which he says "applies the power in a new direction, viz., in the middle of the curve of the forceps instead of the long axis of the handles," which latter he connects together by an india-rubber ring, and while the tractors are being used are always at hand when required, and moving in the direction in which traction ought to be made form a safe guide in what direction to pull.

Having purchased a pair of Mr. Morgan's tractors (made by Maw, Son, and Thompson, London), I endeavoured (more than once) to apply them according to his directions, but besides the great difficulty of reaching the lower edge of the fenestra *after the forceps were locked*, I found that the usual

amount of moulding and elongation of the head had taken place, which forced the scalp into those openings, and made it impossible for me, without injury to the child's head, to apply them.

I have since had a modified pair of tractors made for me by John Whyte, Dublin, which I hope I shall be excused for saying, are, I think, an improvement, as they can be introduced *with the blades and before the forceps are locked*; and not being hooks, but having a shield-shaped extremity, can be buttoned into the fenestra of each blade before introduction, and not being liable to slip, cannot possibly injure the scalp of the child or soft parts of the mother.

When additional traction is required as in cases of convulsions, hemorrhage, &c., where time is very important, I slip the rings of my tractors into an ordinary swivel, attached by metal band to a strap which I have previously buckled round my waist, and thus sitting quietly behind my patient can make any amount of traction *consistent with safety*, by simply leaning backwards, with the greatest ease to myself and without fatigue or perspiring profusely as I often did before and many do still.

The woodcut represents the lower blade of forceps with tractor attached ready for introduction. The amount of force can be modified at will, is always applied in the right direction, and when the operator has no help at hand may save him much fatigue, and sometimes from resorting to the much dreaded operation of craniotomy.—*Medical Press and Circular*, May 7, 1879, p. 378.



78.—THE SHORT FORCEPS SUPERSEDED.

[Mr. BRINDLEY JAMES, of Bermondsey, under this head in the British Medical Journal of March 1, says,]

Having had some little experience in the practice of midwifery, I beg to bring under notice the following method of delivery, in preference to the use of the short forceps. Take, for example, a typical case, to which the short forceps would be used. The head of the foetus is near the outlet of the pelvis, the pains are few and of short duration; the patient is very much exhausted, and seems to have no power of expelling the foetus. Between the intervals of the pains, the head recedes into the pelvis and returns to its former position at the return of the pains, but does not make any progress.

My plan is this. At the onset of the pains, I pass the index-finger of my left hand into the rectum of the patient, and, as it were, hook the head of the foetus, so as to keep it from returning again into the pelvis, till the patient has a return of the labour-pains; and, by continued pressure on the superciliary ridge of the foetus, I gradually expel the head. This method I have adopted in several cases with marked success, and several of my friends have adopted the same method, at my suggestion, with the same favourable results—of course, always premising that the strength of the patient has been kept up by suitable stimulants; and, if thought advisable, ergot have been given to cause uterine contraction.

[Dr. ASHBURTON THOMPSON, writing on this subject, remarks:]

I have used the method of hastening a tardy delivery, described under the above heading in the Journal of March 1st, by Mr. James, for many years past. It affords the most valuable assistance, under certain circumstances; yet I believe he rates its powers too high.

Instead of the index-finger, I use the thumb; for I have found the former quite powerless to prevent the head from receding after it has been advanced by a pain; and I use the right thumb, in order that the left hand may be brought to support it. Without this assistance, no single digit can maintain the continuous pressure which is necessary. When the head, at the *acme* of each pain, just appears at the vulva, the thumb or finger, passed into the rectum, finds a point upon the head where it can obtain a purchase; this point is not the superciliary ridge, although it feels like it, but is a point much above it. By the time the superciliary ridge can be reached, no further assistance of this kind is necessary. It is important to note this; for, if great pressure were applied to that part through the recto-vaginal septum, the eye must be exposed to danger. But the force thus applied can hardly be described as

expellent. Its tendency is towards expulsion, to be sure; but, if it be remembered that it is applied to a point on the head very little in front of the axis of the pelvic inlet, and that the purchase obtained, although sufficient to prevent the thumb from slipping, is not such as to allow of any downward motion, it is evident that its effect must be to press the head against the arch of the pubes, and to prevent its recession after a pain. And that is its use; to fix the head at that point to which it is brought by a pain too feeble to expel it, so that the next is not wasted in carrying it over the same ground again. For success, it depends upon the existence of good uterine power, which distention of the ostium vaginæ is sufficient to call forth; and it is applicable, therefore, to such cases as those in which the natural interval of partial rest, usually observed between the arrival of the head in the cavity and its expulsion, is unduly prolonged. If it be resorted to in a case of atony or real exhaustion, it fails. When the head has thus been advanced, so that the biparietal circumference is embraced by the vulva, it may be expelled by this means, it is true; but that is because extension is all that is necessary to complete its delivery, and a force acting as above, *i.e.*, from below, upwards and forwards, produces this.

Thus the low forceps operation cannot be said to be superseded by this, if judicious and rational practice be had in view. But it is fully capable of superseding it at the hands of those practitioners who are rather anxious to show that they effect ten per cent. of their deliveries with the forceps, than that they use that instrument with any semblance of discrimination. It is quite capable of reducing that percentage to five or even to four.—*British Medical Journal*, March 15, 1879, p. 387.

79.—ON THE INJECTION OF WARM WATER INTO THE VAGINA IN CERTAIN CASES OF LABOUR.

By Dr. WALTER J. KILNER, B.A., London.

Although meddlesome midwifery is rightfully deprecated, yet any assistance which can be given without coming under this designation will be certainly appreciated not only by medical men, but also by the patients themselves. The examples given below will show how injections of warm water into the vagina in properly chosen cases accelerate the labour without causing any increase of suffering to the mother. The only instrument required, besides a bowl of warm water, is a Higgins' syringe fitted with a vaginal tube; but this apparatus can be improved by the addition of a yard of india-rubber tubing three-eighths of an inch in diameter joined to the vaginal tube so as to carry off the water direct from the vagina into a receptacle, thus

avoiding wetting the bed. The water should be as warm as the patient can comfortably bear, and in practice it is advisable not to begin with water raised to the full temperature, but gradually to add boiling water until the temperature of about 105° F. has been attained. The injection requires to be continued from five to twenty minutes according to circumstances. But there is one thing which must be borne in mind, that, unless the injection be given with a due regard to temperature, it is totally useless, so that to avoid disappointment it is better to administer it oneself rather than to leave it to a nurse, unless she can be fully relied upon. The effect caused is the relaxation of the maternal soft parts, and sometimes in addition the labour pains seem to be increased. Besides this, the patients generally say that the injections make them feel more comfortable.

The cases to which this treatment is specially applicable are those in which the os uteri is thin and rigid and the perineum unyielding.

Case 1.—Mrs. A.; third confinement. I was first called in about seven o'clock a.m., and found the os opened just sufficiently to admit the forefinger. Pains every ten minutes. At 3.30 p.m., the os was about the size of a shilling, and the perineum rigid. Pains every two minutes, prolonged. The nurse, under my direction, administered an injection of warm water for about ten minutes, and after another ten minutes I made a vaginal examination, and found the os nearly dilated to the full, and the perineum relaxed. The child was born within forty minutes from the commencement of the injection.

Case 2.—Mrs. B.; sixth confinement. She informed me that in all her previous confinements she was in violent pain from thirty-six to forty-eight hours, but was always quick at the end. About 3 a.m. I found her with pains every two or three minutes, and the os about the size of a shilling, and unyielding. Pelvis roomy, and no difficulty likely to be presented after dilatation of the os. At 8.30 a.m. I was called again, and could not detect the slightest alteration in affairs; so I ordered an injection of warm water to be given her. Half an hour after the injection was administered I was sent for again, but the child was born before I could arrive, although the distance was only two minutes' walk.

Case 3.—Mrs. C.; second confinement. About 2.30 a.m. I was called, and found the size of the os to be about that of a shilling. Pains every five minutes. At 10 a.m. there seemed to be no change except that the pains were more frequent and longer. She was ordered a warm water injection. At 12.30 p.m. I found that the nurse had not administered the injection, and that the os was very slightly enlarged. The perineum was not at all rigid. I then gave the injection myself for about

five minutes, after which the os dilated very quickly. The child was born in forty minutes' time.

Case 4.—Mrs. D.; first confinement. I was called about 10 p.m., and found the size of the os just large enough to admit the finger. The vagina and perineum were excessively rigid—in fact, the most rigid I ever came across. At 12 at noon the next day the os was about the size of a shilling, and the other parts were unaltered. I injected some warm water for about eight minutes, after which the os rapidly dilated, dilatation being accomplished within half an hour, and at the same time the vagina and perineum became relaxed. As the child was a very large one, it took nearly three hours to traverse the pelvis, but the last part of the labour was quickly and easily performed.

Case 5.—Mrs. E., age thirty-seven years; first confinement. At 10.15 p.m. I found the os sufficiently dilated to admit the finger, and seemingly very unyielding. Pelvis fairly roomy, but the vagina and perineum rigid. She was ordered to have an injection for twenty minutes, which was commenced at about 11.15 p.m. A few minutes past one I was again sent for, and arrived just as the child was coming into the world.

Case 6.—Mrs. F.; first confinement. At 11.45 I found that the os would hardly admit the forefinger, although the pains had been continuing regularly since 9 a.m. She took then a draught consisting of forty minims of tincture of opium, which, however, had no soothing effect. At 9 a.m. (twenty-four hours from the commencement of labour) the size of the os was that of a shilling. Ordered an injection of warm water. At 5 p.m. the size was hardly that of a florin. At 10 p.m. the os had remained stationary in size and extremely rigid, although the pains were almost incessant. Upon questioning the nurse, I found that only lukewarm water had been used for the injection, and thereupon I administered one myself for five minutes with water as hot as she was able to bear. After this each pain made such great progress that the os was wholly dilated in fifteen minutes, and the child born exactly fifty minutes after the commencement of the administration of the injection.—*Lancet*, March 29, 1879, p. 439.

80.—PUERPERAL ECLAMPSIA WITH RIGID OS : DELIVERY BY CRANIOTOMY—RECOVERY.

By WILLIAM FEARNLEY, Esq., Lerwick, Shetland.

Mrs. L. G. S., of Lerwick, Shetland, in September, 1876, finding herself pregnant, engaged a doctor rather earlier than usual to attend to her in her forthcoming labour. At this time she believed herself to be about six months advanced. She had

had a miscarriage with her last. On Saturday, September 23, she experienced a severe pain in the left side, and her husband was sent for at once. This was about 2 p.m. About 11 p.m., as I was returning from a friend's house along with a medical friend who was staying with me on a visit, I was called in, and found the patient passing quickly from one convulsive fit into another, and being watched by the assistant of the gentleman whom she had engaged for her labour. The three of us saw the case, and the husband of the lady insisted upon my undertaking the case in the absence on his holidays of the doctor engaged. She was now breathing hard and deeply comatose. From this time till 3 a.m. next morning she had four seizures, and was deeply comatose in the intervals. As the fits succeeded, each seemed more intense than the last—the face being more dusky and contorted each time. During the fits I gagged her teeth to prevent her injuring her tongue, and gave chloroform freely. At 12.30 a.m. I examined per vaginam, and found labour had well commenced, the os being about the size of a shilling, head presenting, the parts roomy and moist. During the intervals of the fits I gave chloral hydrate in thirty-grain doses, but this set up so much sickness that I desisted. The nurse was on the bed, and declared she could always see the fits approach by (1) the twitching of the facial muscles, and (2) the increased stiffness of patient's arms. So sudden were they, however, that the respiration was suspended each time before I could get chloroform applied. Seeing the fits becoming so violent, and the labour making no advance, I dilated the os with Barnes' bags to the size of a crown-piece. Beyond this I could make the os yield not one fraction. The pains were moderate in strength and came on every three minutes. The urine drawn off was scanty, dark, and smoky, and highly albuminous.

At this stage (3 a.m.) I determined on keeping the patient continually under chloroform, seeing that chloral would not remain with her, and that to allow her to come out of chloroform was quite unsafe. This treatment seemed highly promising, as the membranes were unruptured, and the pains, though not strong, were frequent and regular. For five hours under continued anæsthesia all fits were averted, but at the end of that time the most violent seizure of all took place, although she was, to the best of my knowledge, fairly under. During all this time I could never be certain how much of the comparative quietude was due to the anæsthetic, and how much to the deep coma. When the most violent fit of all took place, whilst I thought her safe under chloroform, I was thoroughly alarmed, and determined to empty the uterus, *secundum artem*. Now (8 a.m.) the os was less than a crown-piece and quite rigid. I again applied the hydrostatic bags, but without the least appre-

ciable effect. Turning was out of the question, so I ruptured the membranes and evacuated the contents of the cranium, and delivered by crotchet. Two hours after the uterus was quite empty another fit occurred—the last one.

The patient remained comatose for the next forty-eight hours, and slowly regained her reason. Now it was found she had complete paralysis of the right arm and leg, which continued for some days. On the Thursday (28th) severe jaundice made its appearance, for which she took carbonate of ammonia, &c. This passed off in four or five days.

The patient made a most excellent recovery. The foetus was a six-months one or a little less. Nothing further seems left for me to remark, save a peculiar bloodshotness and pain in the right eye, which produced some little distress for a few weeks.

I had almost forgotten to add that the patient not only was unconscious from the moment of first seizure to the fourth day of attack, but she remembered nothing of what transpired for quite an hour before the onset. She is now, I believe, in capital health, and has been ever since.—*Medical Times and Gazette*, Dec. 21, 1878, p. 705.

81.—ON PERCHLORIDE OF IRON AS A STYPTIC AND CAUSTIC IN GYNECOLOGICAL PRACTICE.

By Dr. J. MATTHEWS DUNCAN, LL.D., Obstetric Physician to
St. Bartholomew's Hospital, London.

The occasional occurrence of death as a result of the practice, while it makes resort to it a matter for utmost care and consideration, does not, however, completely and at once, forbid its use. Some practitioners may still feel justified in applying it as a last resort: in the meantime, at least, I do not, preferring to rely on preventive measures and the use of ergot, pressure, kneading, cold, and heat. The danger of artificial anæsthesia does not prevent my resorting to it, but the danger of iron injection is, I feel sure, much greater; while there are, to replace it, more than one alternative remedy of equally well if not better founded reputation.

Regarding the condition of the bleeding surface in a recently delivered woman, with its numerous large open sinuses, one would naturally dread the passage of the iron solution into the vessels, and the post-mortem investigations in fatal cases prove that it does so pass. They illustrate the passage also of other noxious septic or aseptic matters, whose baneful progress along these vessels, and into the general circulation, it might be difficult to demonstrate otherwise so indisputably. The case which I am about to narrate shows that, for this passage in the case of solution of perchloride of iron, it is not necessary

to have large open sinuses, such as are found after delivery or in wounds, for in that case there were only such very small open vessels as the eye cannot discover, but which may be regarded as existing in a cancerous sore which bleeds, though only slightly.

An active expanding power in the uterus, "sucking up," has been imagined, and may be applied to explain the entrance of uterine contents into the sinuses; but I know no evidence of the existence of such a power. That the uterus, when not in contraction, may be expanded, is well known, and on this subject some inquiries, as yet too fragmentary, have been made. But such expansion must be active in order to produce the desiderated force, and I know nothing that induces belief in such active expansion. In the retentive power of the abdomen, or in the condition of negative pressure, as it is called, in that cavity, there is a sufficient explanation; and of this adspiratory abdominal force there is abundant proof.

It is a matter of course that, while bleeding is going on from the sinuses, there can be no sucking into them; but the one action may succeed the other, or repeatedly alternate with it.

Like most, if not all, gynecologists, I have for a long series of years been in the habit of using locally strong and weak solutions of perchloride of iron, generally the former, in various ways, with various objects in view; and, except occasionally much pain and a very rare adhesive perimetritis or phlegmasia dolens, I have seen, amid advantageous results, nothing to counterbalance the good, till the case, to be now related, occurred. As a styptic in all extreme forms of uterine bleeding, and in connection with early abortion and uterine diseases and operations, and in vaginal and vulvar diseases and operations, as a caustic in endometritis, I have applied it without stint. It is recommended, in most works on gynecology, to be used in such ways, and in others, as by injection into cancerous tissues. I have always injected it gently, that is under slight pressure, and have used little at a time, not more than a drachm. If more was used, I have paid attention to the previous dilatation of the cervix.

Von Haselberg, Barnes, and Kern have related cases of passage of the iron solution through a Fallopian tube, but of this I have seen no example. In Von Haselberg's case the occurrence was explained by the open condition of the tube. Klemm, whose work, *Ueber die Gefahren der Uterin-injection*, is referred to by Schroeder, but which I have not seen, is the only author whom I know that refers to the danger of injected fluids passing into uterine sinuses, and then into veins in the broad ligament. His data seem to have been the result of experiments, and are no doubt of great value.

The following case demonstrates the passage of the iron solution through the uterine sinuses into the veins of the broad ligament and the mode in which death was produced. In consequence of it I intend now to make use of tincture of iodine, hoping for results like those of the iron solution without its attendant dangers. I give a general statement of the case.

The woman was suffering from ulceration of cavity of the body of the uterus, which was supposed to be malignant. She had much bloody and watery discharges from the vagina, and was very anxious that some attempt should be made to stop it and cure her. The uterus was enlarged and retroflected, the cervix large and patulous. Through the latter, after dilatation, the finger could be passed into the cavity of the body of the uterus. Nitric acid was first used, and subsequently the iron solution. It was the second injection of the solution of perchloride of iron that was followed by disaster. Till this, benefit of a temporary kind was gained from the injection.

After the second injection, which was made with the greatest ease and gentleness, she had pain in the hypogastrium of great severity, with weakness and symptoms of extreme prostration, yet she was able to walk from the operating theatre to her bed in the large ward. Now came on the most alarming appearance of impending death, which I said to the bystanders could be accounted for only by embolism, the abdominal pain and every indication of peritonitis being absent. But I could not account for the embolism. I had not then any knowledge of Klemm's experiments. Under treatment she partially rallied. Her death was sudden, and quite unexpected at the time by the very intelligent sister, who happened at that minute to be standing by, engaged in conversation with her. Death occurred nearly two days after the injection. [Dr. Duncan then gives the case in detail.]—*Edinburgh Medical Jour.*, Feb. 1879, p. 673.

82.—ON TENTING.

By Dr. FRED. C. COLEY, London.

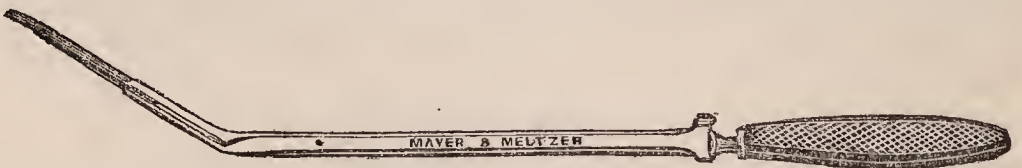
The operation of dilating the uterus with tents is one which is often difficult, and always more or less painful and dangerous. The object of this paper is to explain a few simple contrivances whereby the difficulties (and, therefore, to some extent, the pain and risk) may be diminished.

When the uterus is to be *fully* dilated, it will often be most convenient to commence with laminaria, and when the os internum is sufficiently patent to admit the point of the index, to complete the dilatation with sponge tents. I shall assume this course to be pursued, although no doubt it is sometimes preferable to carry out the whole process with laminaria.

It is a good plan to commence by dilating with graduated sounds. As a rule (open no doubt to a good many exceptions) a normal uterus can be dilated with sounds, even at one sitting if necessary, up to No. 12, without the use of any force involving danger, or even much pain. The usefulness of this is obvious. A larger tent, or two instead of one, can be used, and so a start is gained, and the operation may be completed, perhaps in one sitting less—no slight advantage, as it means that the patient has so much less time to be exposed to the pain and danger of tenting. Of course, in many cases it is unnecessary, the uterus being already morbidly patulous enough to admit a tent larger than No. 12.

In cases of fibroid, and in some cases of acute flexion, the difficulty of introducing a tent consists chiefly in the crookedness of the uterine canal. If, under such circumstances, after dilating with graduated sounds, a small ordinary uterine sound be passed—or better still, Sims's uterine probe—a laminaria tent may be easily slipped in beside it, the uterus being straightened by the probe, on withdrawing which a second tent can be introduced in its place.

In ordinary cases, Dr. Barnes's tent introducer, made on the pattern of a catheter cut short, with the stylet projecting, is very convenient. But in difficult cases, where the uterus is distorted, and considerable power of directing the point of the tent is required, it seems too feeble. It easily bends. If the strength of the spike were increased, the tent would have to be weakened by enlarging the perforation. The plan above described to some extent evades the difficulty, by getting rid, for the time, of the distortion. But it is not always applicable. To meet this difficulty I contrived the forceps shown in the figure.



It is made by Messrs. Mayer and Meltzer, of Great Portland Street. The inside of the blades is furnished with small teeth, like those of a rasp. It closes with a simple catch, so that the hand is not fatigued, nor the attention distracted by the effort of holding it shut. The tent is held so firmly that it forms with the forceps practically one instrument, which bears a general resemblance to a uterine sound with Sims's handle. I believe that with this forceps a tent can be inserted the whole distance in any case where a sound can be passed. Of course it can be used either with solid or hollow tents. I have found it very convenient, especially in difficult cases.

A great difficulty with laminaria tents is their tendency to

slip out. They do not fall out. They are extruded. This is especially liable to happen when the uterus is flexed. The tent is seldom found quite free in the vagina, but with its point just below the point of flexion: with the cervix well dilated, but the os internum in *statu quo*. It is often recommended to retain the tent by plugging the vagina, but this is objectionable, as it increases the risk of septicæmia, by retaining the discharges. And it is often ineffectual, for if the uterus has much tendency to extrude the tent it will do so, maugre the plug. It is generally recommended to choose a tent half an inch longer than the uterus. But if a tent be taken about one-third inch less than the length of the cavity of the uterus, it can be passed quite into the uterus. A second longer tent should then be passed, if possible the whole distance, otherwise just into the cervix, to dilate the os externum. It will not be extruded, for No. 1, resting with its base just within the os externum, is kept in place by it, and keeps the uterus straight and extended, so that it has no tendency to force out No. 2. In ordinary cases, if there are two tents in the os internum, and one in the os externum, the two ora will be about equally dilated when the tents are removed; because the os internum is much more contractile than the os externum. When a laminaria tent is removed it often looks as if a string had been tied round it at the os internum, while the os externum has left little impression upon it. There is, therefore, no risk of a tent being incarcerated by the os externum.

Before attempting to introduce No. 2, No. 1 should be withdrawn by the string, till it slightly projects from the os externum, otherwise the point of No. 2 will hitch against the base of No. 1. Provided care be taken to keep the point above the inner os, it can be easily replaced by the finger, as soon as No. 2 is *in situ*.

During the whole time that the uterus is being dilated, the vagina should be syringed with warm antiseptic lotion about every three hours. This lessens the risk of septicæmia, eases pain, and facilitates dilatation. It is well also to thoroughly syringe the vagina each time that the tents are changed. This is done most safely with the help of a speculum.

I think that the best instrument for introducing sponge tents is a simple stout spike with a shoulder to prevent the tent from being jammed upon it.

Sponge tents are often very disappointing, dilating the cervix largely, and leaving the os internum in *statu quo*. The base swells first, and draws the point out of the inner os. This may be prevented by a disposition somewhat like that recommended for laminaria tents.

But some of the most annoying difficulties connected with

tenting occur when the tents have to be withdrawn.

The loop of silk which the instrument-makers attach to the tent for this purpose is usually so short and thin that it is seized with difficulty, and breaks if the tent is held at all firmly by the uterus. A piece of whipcord, sufficiently strong, but not clumsy, should always be substituted. It should be long enough to reach one or two inches out of the vagina. This will save the surgeon a good deal of trouble, and the patient some pain.

Sometimes, however, when the os internum is very rigid, the string will tear out of the tent, and there have been cases in which the tent has broken in halves at the point of constriction. Of course, the only plan in such a case would be to dilate with another tent (which would be rather difficult), and then remove the incarcerated piece of the first one with forceps. But the accident is easily avoided by simply altering the manner of attaching the string. Let a hole be bored through a hollow laminaria tent, about half an inch from the point. Let the two ends of the string be passed inwards through this hole on the opposite sides of the tent, and brought down through the hollow of the tent. The middle of the string will then go half-round the tent on the outside, and the ends will hang out at the bottom. There cannot then be any difficulty in removing the tent, if the string be reasonably strong. It will not at all readily cut through the tent, because it is placed across the grain. Nor does it, so placed, materially increase the difficulty of introducing the tent, if carefully drawn quite tight.

A similar difficulty occurs much more frequently with sponge tents. The string, as usually attached at the base of the tent, easily tears out. As a rule, under like circumstances, a laminaria tent may be readily removed with forceps. But sponge seized with forceps easily tears, so that the tent has to be removed piecemeal, the operation being more like a complicated case of craniotomy than anything else.

Thomas and others recommend that the string should be passed through the tent lengthwise. But then, if the os internum were much contracted, the part of the tent above it would be bulged out by the downward pull taking its bearing from the point, and so the resistance would be increased. Under these circumstances, it is not at all unlikely that the string would now and then tear its way out.

I have successfully used the following plan: The string is passed through the tent about half an inch from the point. Half an inch below, another hole is made, and the ends of the string passed through it in opposite directions. This is repeated till the base of the tent is reached. The string is thus laced through and through the tent from within half an inch of the point. The first strain of the pull is always upon the base,

because of friction, so that there is no bulging of the tent above the constricted point, although the string has a hold upon the whole length of the tent.

I have had tents made by Messrs. Krohne & Sesemann, with this arrangement of the string. It does not practically interfere with expansion.

The suggestions which I have ventured to make in this paper are simple, even to triviality. But, to the best of my belief, they are not to be found in any of the ordinary text books. And I think they will be found practically useful.—*Obstetrical Journal*, April 1879, p. 14.

83.—CHRONIC INVERSION OF THE UTERUS REDUCED BY MEANS OF ELASTIC PRESSURE.

By Dr. JOHN WILLIAMS, University College Hospital.

Mrs. H., aged twenty-six, was admitted into University College Hospital August 30, 1878. She was the mother of three children, the last being two years and four months old. She said that her last was a difficult labour, and that the doctor stated the womb came down after it, and that he replaced it. She did not menstruate for twenty-two months after her confinement, but she was regular every four weeks for the six months immediately preceding her admission into the hospital. The flow was very profuse, and latterly had so much increased that she had become very weak. She had had no pain with it. She had had a profuse yellow discharge since her confinement. When first seen she was extremely anæmic, almost bloodless. On vaginal examination a tumour was found projecting into the vagina—thought at first to be a polypus. It was of a pyriform shape, and about the size of the normal uterus. Further examination, however, showed that it was attached all around to the rim of the cervix uteri, no uterine canal could be discovered, and finally, no uterus could be found above in the pelvis. It was concluded, therefore, that the tumour was formed by the inverted uterus. The bowels were confined, and a dose of aperient medicine was given. On September 3rd, at 2 p.m., a cup of vulcanite mounted on a metallic stem, having a perineal curve similar to that described by Dr. Aveling, was introduced. The cup was applied to the inverted fundus, and to the ring at the end of the stem were fastened four elastic bands, two of which were carried anteriorly between the thighs in front of the abdomen, and fastened by tapes to a broad strap of adhesive plaister placed round the waist, the other two elastic bands were carried posteriorly and fastened by similar means to the adhesive strap in such a manner as to cause the force exerted upon the fundus uteri to act, as far as could be judged, in the

axis of the brim of the pelvis. At this time the temperature was 99.6° F. The patient complained of much pain, and a pill, containing 1 gr. of opium, was given. A second pill was given in the evening, but the patient vomited it. The pain continuing, a suppository, containing $\frac{1}{2}$ gr. of morphia, was administered. The patient had a fair night, slept a little, and complained no longer of pain. On the following day, September 4th, there was a copious serous discharge from the vagina. At 10 a.m. the instrument was removed, and examination showed that the uterus was in part re-inverted. A distinct rim of cervix could be felt with a well-marked fossa around between it and the tumour. The instrument was replaced and the elastic bands tightened. At 9.30 a.m. temperature 100° F., pulse 96 per minute, at 2 p.m. 102° , at 4 103° , pulse 110. At 11 p.m. the cup of the instrument was found to have penetrated through the canal of the cervix, and to be completely lodged in the cavity of the body. Much difficulty was experienced in withdrawing it, owing to the resistance offered by the cervix. It was ultimately withdrawn by tilting the cup and pulling it out sideways. The uterus was found to be completely replaced. During the day the discharge continued in considerable quantity and became offensive. Vaginal injections of hot water were used three times a day. On September 5th the temperature rose to 104° F. and the pulse to 120 per minute. There was no shivering, pain, or tenderness. The discharge gradually diminished and became less offensive, until the 17th, when it had almost ceased. The temperature gradually went down to 101° F. on the 6th, and became normal on the 10th. After the first day the patient complained of no pain. She continued very anæmic though taking iron for more than a fortnight. On September 17th she was ordered a hypodermic injection of dialysed iron (M iij) once daily. She then rapidly improved and gained in strength and colour. On the 24th she was made an out-patient. The hypodermic injections of iron were continued twice a week for a month, when she had recovered her strength and colour.—*Obstetrical Journal*, April 1879, p. 21.

84.—CASE OF FIBROUS TUMOUR OF THE UTERUS, COMPLICATED WITH INVERSION; REDUCTION OF THE INVERTED UTERUS.

By Dr. J. MATTHEWS DUNCAN, F.R.S.E., Obstetric Physician to St. Bartholomew's Hospital.

[The patient, æt 38, had been seventeen years married, and had had two children. Health good until quite recently, when menorrhagia, dysuria, pain in the back had drawn attention to the pelvic organs.]

Above the pubes a flattened hard mass, neither sensitive nor tender, dull on percussion, reaches to the level of a line joining the iliac spines. The vagina is occupied by a hard rounded tumour, at least as big as the fist. It is lying in a copious, bloody, putrescent fluid. The uterus cannot be made out, being inaccessible on account of the bulk of the tumour.

A sloughing fibroid was diagnosed, and it was resolved at once to remove it.

The tumour was seized by a pair of powerful volsellæ, and pulled through the vulvar opening of the vagina. Its lower extremity was nearly black and gangrenous, the dead tissue hanging partly in long shreds. The condition of the uterus was now investigated. The finger examining at the site of the external os uteri easily diagnosticated complete inversion. Below the cervix uteri, and on to the gangrenous end of the tumour, the whole elongated mass was of uniform or nearly uniform thickness or circumference; the uterus being greatly hypertrophied. Some difficulty was experienced in deciding the limit between the fundus and the tumour attached to it. At one part a slight furrow was felt and seen. Into this the point of a finger was pressed. The tissues yielded, and the fissure was artificially increased, till it became plain that the line of attachment of the tumour to the fundus uteri had been struck. Enucleation was now completed. The tumour was for the most part bare, its encapsulating tissues having sloughed away. But a flap of capsule, as large as a crown piece, and fully one-third of an inch in thickness, was left attached to the fundus till it was cut off by scissors.

The operation was performed under the influence of ether on the 16th March. There was no bleeding worthy of special notice. The uterus was replaced in the vagina. Persistent and very powerful reducing taxis was now in vain used by myself, Drs. Godson, and Champneys. It was, as is usual, if not invariable, easy to get the body within the cervix, but the reinversion of the body itself proved unattainable at this time.

She soon recovered from the immediate effects of the operations to which she had been subjected. The fetor of the discharges soon ceased, but a blood-stained flow persisted. Her urination became natural and easy.

After ten days the pulse and temperature became natural, the bloody discharge ceased, there being only a slight purulent flow. She was allowed to get out of bed. For several weeks now she was carefully fed and treated with iron. Her health improved greatly, and more progress would have been made had there not occurred repeated hemorrhages of varying amount, which necessitated horizontal rest in bed.

On the 21st May an attempt was made to produce reduc-

tion, the instrument being kept on for seventeen hours. It was removed on account of the abdominal pain, the vomiting, and the general distress which it caused. On its removal, the body of the uterus was found to be pressed up within the cervix. The instrument used was the same as I had used in Edinburgh several years previously. It resembled an ordinary stethoscope with an ear-piece, the other end being cup-like and larger than is ordinarily seen in a stethoscope. The cup-like end was adapted to the fundus uteri. The other end was attached by india-rubber straps to the horizontal limb of an ordinary T bandage. These india-rubber straps were put on the stretch, and this tension was the reducing force. By simple dynamometer experiments it was found that the force applied to the uterus through the cup was between one and two pounds. This force is very small compared with that exerted in the forcible taxis when the tumour was removed; but it was confided in, and proved successful, not as a pressure sufficient to force reduction at a sitting, but as a gradually acting reducing power.

When the attempt at gradual, not forcible, reduction was resumed, the repositor was improved by being made shorter, and two were at hand, one with a larger cup to replace within the cervix, another with a smaller cup to follow up the fundus after it had retired within the cervix. In the next case which comes under my care, I propose at one sitting to replace within the cervix, and at once apply the smaller cupped repositor. This plan appears likely to effect a saving of time and trouble, and consequently of suffering. The operations were done under the influence of anæsthetics, and opiates were used to assist the patient to endure the continued pressure of the instrument.

It has been thought advisable by Aveling to give the stem of the repositor a perineal curve, such as is observed in Tarnier's midwifery forceps, and for reasons analogous if not identical. But such modification of the simple straight repositor seems to me unnecessary. The Tarnier forceps is made with a view to the easy and safe extraction of a large body which ought, or indeed must, descend and emerge in the axis of the cavity and outlet of the pelvis, or in the circle of Carus, as it is called. But the uterus is small, and therefore the direction of the action of the repositor is not a matter of any nicety; and, moreover, the range of the repositor's action is in the brim of the pelvis, and so limited as not to demand the same consideration as is demanded by the range of the action of the midwifery forceps.

On the 28th May a second attempt at reduction was made. It was done as the former was, and failed from the cup slipping off the fundus uteri.

On the 4th June the mechanical treatment was resumed. On the 6th the fundus was found just within the cervix; and after removal of the other, the smaller cupped instrument was applied. At 5 p.m. on the 7th, about twenty-six hours after the use of the smaller instrument was commenced, and seventy-four hours from the commencement of the replacing pressure, the patient felt a sudden and severe pain in the hypogastrium, and says she felt the instrument slip into her body. The pain was worst after she felt the instrument slip. She was sick twice, and felt faint for about three-quarters of an hour. Brandy and morphia were given. On the 9th, at 3 p.m., the instrument was with some difficulty removed from the uterus in whose cervix it was incarcerated. The same evening she sweated profusely, and complained of pain and numbness in the left leg. These feelings in the leg were less next day, and soon disappeared.

For some days she had much bilious disorder, and occasional sickness and vomiting. A purulent discharge from the uterus was abundant at first, and very slight when she left the hospital. On the 18th June the uterus measured three inches. She soon began moving about the ward, had a hot bath, and her anæmic appearance diminished. She still had occasional attacks of sickness, and complained of pain in the back. On July 2 she left the hospital in good spirits.

Three weeks after leaving the hospital her death was reported; but no satisfactory account of the cause of it could be obtained. At the time of death her anæmia appeared to the physician in attendance upon her to be extreme. She suffered from anorexia and constant vomiting. There was œdema of both feet, and the left leg was also similarly swollen. This swelling was described as coming on suddenly with rigors. There was fluctuation in the abdomen.

There appears good reason to believe that the death was the termination of the morbid conditions, a part of whose history is given in this report. Probably extreme anæmia was the chief element in the causation of the fatal event.—*St. Bartholomew's Hospital Reports*, vol. xiv., 1878, p. 97.

85.—ON PAINFUL SITTING AND COCCYGODYNIA.

By Dr. J. MATTHEWS DUNCAN, LL.D., &c., Obstetric Physician to St. Bartholomew's Hospital.

We have several cases illustrating this subject. In some of them painful walking accompanies the painful sitting; indeed, in the last disease that I shall mention to you, painful walking is more important than the painful sitting. Painful sitting is as good a name for a disease as dysmenorrhœa is, and quite as

distinctive; but painful sitting is not a disease, nor is dysmenorrhœa—both are symptoms.

The first special cause of painful sitting that I have to consider is inflammation—not affecting the external organs, not affecting the vagina, but affecting the deep-seated genital organs, the uterus and ovaries. This is not an infrequent cause; and the first case I am to read to you is a good example of it, an apposite example for us, because the poor woman came to the hospital declaring that she could not sit—that was her complaint; for her that was the disease. I shall read her case:—“M. C., aged twenty-seven; admitted November 16; married ten years; has had four children—the youngest two years old, two alive; the others died during teething. Has had no miscarriage. Catamenia began at thirteen years, and has generally been regular; nothing abnormal noticed about any of the later periods. About six weeks ago she was suddenly attacked with a severe pain in the right inguinal region, which has been present ever since when sitting. The pain is hardly felt at all when standing or lying down. This pain she ascribes to a kick on the belly; and I think it is a very probable explanation of it. About the middle of this term of six weeks she had a scanty thin brownish fetid discharge, which has since subsided, and is now imperceptible. It lasted for a week or a fortnight. The pain is identified by pressing on the perineum, and subsequently by pressing the uterus digitally. The cervix uteri is nearly in its natural situation, patulous and hard, admitting the finger easily. It has an irregular hard internal surface. The uterus is fixed. The whole roof of the pelvis presents hardness, or dense fulness, which is tender.” Now you can easily, from the record, make out that this woman has cancer of the neck of the womb. She knows nothing about that, and I believe does not suspect it; she thinks her disease was caused by the kick she got from her husband, and as for her the disease is painful sitting, I think she is quite right as to its cause. Somehow or other this kick was connected with an attack of perimetritis, an attack of inflammation around the womb, inflammation affecting the serous membrane, inflammation leading to the fixation of the uterus which we found; and, so far as her disease consists in painful sitting, this inflammation is the cause of her disease. Attacks of inflammation, apart from violence, are quite common in connection with cancer of the womb. This woman’s sufferings are caused by inflammation around the womb in the early stage of cancer of its neck. Now, I wish you to observe how clearly in this case the nature of the disease was made out. Firstly, a cancerous uterus is not a tender one. This woman’s uterus was not tender where it was cancerous; it was the neighbourhood of the uterus that was the seat of the

tenderness, the seat of the inflammation. When the perineum of this woman was pressed by the hand, while she was lying on her side, she at once recognised the pain of sitting. She felt the same pain as when the perineum was pressed upon by her seat. She had not the pain when she lay down or when she was standing. Following up the pain, the finger was introduced into the vagina, and found the same pain was produced by pressing upon the inflamed and tender parts near the womb. There could thus be no doubt of the nature of the disease. Of this part of her disease—which, unfortunately for the woman, is not the major part—she will get rid by suitable treatment, especially by continued lying in bed. She is, indeed, already nearly well.

In connection with this case I have, lastly, to state the theory of this painful sitting; and a very easy experiment explains it. It is not generally recognised that the bowels are pressed upon by sitting; but it is a fact, as this case illustrates. When a woman sits upon a seat, the pressure upon her hips, even although the deeper parts are protected by the tuberosities of the ischia, communicates pressure to the deepest parts in the pelvis; and, if those parts are tender, pain is the result. The experiment that I alluded to, as demonstrating what I have just said, is to place the hand upon the hypogastrium while the perineum is exposed. If you press with it in the direction of the axis of the brim, you push down the perineum and the hips. A very slight pressure upon the hypogastrium makes the perineum bulge, makes the hips descend. Of course, when the hips are pressed upwards, or the perineum is pressed upwards, you have an influence which is, in like manner, communicated back to the hypogastrium, and thus you have pain. This is illustrated in many cases of ovaritis and metritis of all kinds. This part of painful sitting is a separate thing from the injurious influence of sitting. That I am not speaking about. The injurious influence of sitting is a subject I may illustrate at some other lecture. What I am speaking of now is painful sitting, and the injurious influence of sitting is a much wider subject than what I am now considering.

I go on, now, to another set of diseases connected with the coccyx, which diseases are not peculiar to women, but are, I believe, much more frequent in women than in men; and they have got a collective name, which is also an artificial, not a pathological name—Coccygodynia. Now, the pathology of this department of painful sitting is so far advanced that I recommend you to give up the use of this name except as a proper word to express pain in the coccyx, for which no further explanation can be given. That is to say it is a neuralgia; perhaps not a pure or simple neuralgia, but yet a neuralgia; and a

neuralgia is in the majority of cases a disease of which no further explanation can be given. Indeed, many of the cases usually included under coccygodynia are not diseases of the coccyx at all.

Occasionally the coccyx is the seat of inflammation; or its periosteum gets inflamed, and you have abscess around it. Of that disease I have not seen an example, but I have seen enough to show me that such a disease may exist. I have, for instance, seen a periosteal abscess extending from the point of the coccyx to the base of the sacrum, the whole length, which shows that such a disease as inflammation and abscess of the coccyx may occur. There is no doubt, indeed, that it has occurred. But the commonest cases of neuralgia of the coccyx or of true coccygodynia, although they have tenderness, or rather sensitiveness, as a symptom, have no inflammation, no inflammatory tenderness. Now, this disease is common, and it is common in men as well as in women. I have seen cases of it in men, although I come very little in contact with that sex. In men it generally arises from constipation or some disorder of the rectum, such as hemorrhoidal congestion. I may mention an example of it as it occurs in women. A young lady in her first pregnancy, enjoying perfect health, was sent to me, only two days ago, by her husband, because she could not sit. When she came into my room she laughingly said it was an absurd complaint, but she could not sit. It was easy to make out that she had this tenderness—not inflammatory tenderness—this sensitiveness, rather, of the coccyx. Now, this disease is generally easily cured, or rather it goes away, and the treatment of it is scarcely worth describing. It is the use of laxatives, hot bathing, sedative applications. In a severe and persistent case you might try the hypodermic injection of morphia, and it has been said to cure the disease. Whether it has done so or not I shall not answer for; perhaps time would just have cured it equally efficiently. The disease is essentially a come-and-go one, and it is very difficult to judge in such diseases what is to be attributed to treatment and what to time. Now, these are the commonest cases. Other cases, however, are not rare; they arise chiefly from injury, and they seem to affect the sacro-coccygeal joint, and still more its ligaments, and the sacro-sciatic ligaments especially. Of this affection I shall give you an excellent example. Mrs. L., aged thirty-two, married for two years, had her first child nine months ago. During the second stage of labour she had intense suffering, especially during pains, in the region of the coccyx, where she has still all that she complains of. Ever since her confinement she has had the pain very severely during defæcation; but now it is less than it was at first. When she began to get up, sitting brought on the pain, and she had to give

it up entirely; but lately the pain in sitting has diminished, and now it is entirely gone. On examination the coccygeal region is easily identified as the seat of all the pain. There is no swelling nor dislocation of the bone. Pressing on it increasing flexion, as in sitting, causes no pain; but extension so as to bring the least tightness of the sacro-sciatic ligaments brings on the well-known pain. Pressure on the ligaments to tighten them also induces the pain.

This is a very clear case, and the disease is gradually disappearing. The only pain that remains is produced by stretching the sacro-sciatic ligaments. I have no doubt that the disease in this woman is some sort of inflammatory rheumatic condition of these ligaments. Neither have I any doubt that she will get quite well: she is in the process of getting well. You observe that, in this case, the disease was brought on by injury sustained during labour. This intelligent woman's account of the second stage in her labour, and of the pain in the coccyx, leaves no room for doubt that then the disease was produced. The pain she suffers now is the same she suffered then—only much less. This case I recommended to be treated by hot bathing, by keeping the bowels easy so that large masses of fæces might not descend and cause great extension of the coccyx. No more treatment was demanded, because the case was progressing slowly in a very satisfactory manner. Should it prove obstinate, I would be inclined to recommend to this woman to have the sacro-sciatic ligaments divided at their attachments to the coccyx—an experiment which is well worth trying.—*Medical Times and Gazette*, Nov. 30, 1878, p. 619.

86.—ON RELAXATION OF THE JOINTS OF THE PELVIS.

By Dr. J. MATTHEWS DUNCAN, LL.D., &c.

I have a few words to say upon a condition of the joints of the pelvis which is rare as a disease, and which interferes with sitting and walking, especially the latter—that is, relaxation of the great essential or intrinsic joints of the pelvis, the symphysis pubis and the two sacro-iliac joints. These joints in the end of pregnancy become naturally loose and juicy, and a considerable increase of motion is permitted in them. The loosening of these joints becomes morbid very rarely. When morbid, it has been found sometimes, in a few recorded cases, to be so extreme as to produce hopeless lameness. The joints have been so relaxed, and present such an amount of mobility, that by no contrivance can they be fixed so as to enable the woman to stand. Cases of that kind are among the greatest of rarities, but cases of slight extraordinary loosening are not very rare. They are recognised or diagnosed with great

difficulty. You are led to suspect the existence of the condition by finding that the disease dates from pregnancy; it may be not from the first pregnancy. The last case which I saw was a case beginning in the second pregnancy. The next thing that leads you to suspect the disease is to find pain complained of in the symphysis pubis, or in what the patient calls "the bone" in the mons veneris, and in the two sacro-iliac joints, or in one of them. The pain of the symphysis pubis and in one of the sacro-iliac joints almost invariably go together, but both sacro-iliac joints are not invariably affected. What is the difficulty of recognising this disease? There is no difficulty in a case of extreme relaxation. Then, the woman can find the disease out for herself; but in a case of slight relaxation it is a matter of very great nicety, and you have frequently to put the woman through a variety of evolutions before you can satisfy yourself that these joints are moving. I have found it generally vain to try to make this out in the symphysis pubis, partly on account of the disagreeableness of the proceedings. The proceedings are extremely indelicate; but only in a certain sense, for there is nothing truly indelicate that forms part of a duty; but they are extremely unpleasant, and the word "indelicate" implies a part of the unpleasantness. Besides, when I have attempted to diagnose the movement of the symphysis, I have been extremely ill satisfied. In the case of the sacro-iliac joint there is no embarrassment; the difficulty is in being quite sure of the movement. In a healthy woman you can make out no movement. You start from that. If, then, we find distinct movement, we may be sure that there is this morbid condition. This distinct movement is to be ascertained by seizing the haunch-bone, and, while the spine is fixed, trying to make it move a little; or you fix the haunch-bone, and make the woman move her spine; and then you can see or feel, while the haunch-bone is fixed, a distinct movement of the spine upon it by making the woman change her position. I advise you not to be sure you make it out till you have perfectly satisfied yourself. Supposing you make it out, is there anything to be done? Like many others, this disease is fortunately frequently spontaneously cured. It is natural to expect that, as in a cow the moving huckle-bones get fixed again after parturition, so in a woman the movable haunch-bones will get fixed after parturition; and the same may happen more slowly in the extraordinary or morbid cases I am describing. In such cases I have always encouraged a woman to walk, to brave out the pain if she could, because the irritation produced by the walking may conduce to the refixation of the joint. Cases of this kind do get better. The bones do get fixed again. Until they get fixed there is one means which is of great value,

that is, a very firm bandage around the pelvis. You give an artificial fixedness. Now, you will find it very difficult to get a woman to wear this bandage, because it is extremely unpleasant in itself; and it is only after she has found the advantage of it that she will consent to wear it. The bandage must be made, not of ordinary bandage materials, but of horse-girth stuff. This is put round the pelvis, and strapped as tightly as the woman can endure; and if it is to be of any use it must be inconvenient, because, in order to be fixed upon the proper part, it must descend to a considerable extent upon her limbs—that is to say, it must come down to, or even a little below, the trochanter major,—and this makes walking very disagreeable. I have seen cases in intelligent women where I can have no doubt of the real advantage of this bandage—where, indeed, the woman could not walk without it.—*Medical Times and Gazette*, Nov. 30, 1878, p. 621.

87.—REMARKS ON OVARIOTOMY.

By Dr. THOMAS WHITESIDE HIME, B.A., Medical Officer to the Hospital for Women, &c., Sheffield.

I have been in the habit of giving all my cases for some time before the operation sulpho-carbolates in considerable quantity, including the salts of iron, quinine, sodium, calcium, &c. Being a strong believer in the efficacy of the antiseptic treatment of wounds, which I first employed in ovariectomy in 1876, it has seemed to me desirable to administer internally such drugs as present at least the probability of checking, if not preventing, the septic process. It has been shown by experiment that the flesh of animals killed after a short course of sulpho-carbolate of sodium showed a marked tendency to resist putrefaction, and that urine from a patient similarly treated resisted putrefaction. Further, the odour of carbolic acid has been detected in the breath of patients taking sodium sulphocarbolate. It seems certain, therefore, that this salt is quickly absorbed and carried through the system of living animals, the double salt being decomposed in the blood or tissues, and the carbolic acid eliminated by the lungs, and probably also in part by the kidneys. Thus, a large quantity of carbolic acid can be introduced into the system, amounting in large doses to as much as 100 grains in twenty-four hours. To administer this quantity of acid in direct doses would be impossible; and further, the salts of iron, quinine, &c., are in themselves very often indicated for patients suffering from ovarian dropsy. It seems to me, therefore, that these drugs are well deserving of a trial. My own experience is strongly in their favour. It is often advisable to employ nutrient enemata,

in addition to feeding by the mouth, in the case of emaciated patients being "made up" for operation. Liebreich's pepsin-essence will be found very valuable in assisting gastric digestion. The usual enemata of beef tea, milk, &c., thrown into the rectum with an ordinary enema-apparatus I have found useless. I now invariably use Leube's formula, letting the fluid gravitate far into the bowel above the rectum, by means of an apparatus. By this means, digestion, absorption, and the evacuation of healthy motions may be secured, and patients fed for weeks, even when nothing can be taken by the mouth. The advantages of the gravitation-method over all instruments I have ever seen are very great. Even when the rectum allows everything injected into it to escape, if the fluid be allowed to gravitate slowly into the bowel through a tube passed high up, it will generally be retained.

I have added to the spray-producer I employ (which has three jets, and produces a fine spray more than three times as large as the ordinary Lister's apparatus) a second gas-burner, in order to be able to warm the fluid which is to be atomised. The cooling produced by the exposure of the intestines and peritoneal cavity to an ordinary spray during an ovariectomy, which may last two hours or more, is a serious source of danger. I think the second burner is, therefore, an improvement, as it considerably raises the temperature of the spray. I have, in my last three cases, used thymol, in spray, solution, and gauze, and with the best results. I have used solutions of different strength, freely introducing it into the abdominal cavity on sponges, &c., without any ill effect. In general it is best to make the incision in the *linea alba*, which, however, is not always in the median line of the body. Should the incision have been made into the rectus muscle, it will be much better to keep to it than to spend time in searching for the *linea alba*, and, by poking about in the muscle, probably lead to the formation of an abscess and the burrowing of pus along the sheath of the muscle. Indeed, Storer considers that there are advantages in making the incision through the muscle rather than in the *linea alba*, as being more likely to heal *per primam intentionem* and form a sound cicatrix. It is better to cut through the abdominal wall with a free hand, thus securing cleaner cut edges to the wound than will be got by more careful dissection. Should the cyst-wall be adherent to the peritoneum in the line of the incision, great trouble may be experienced in distinguishing and separating them. By extending the incision upwards, one may get beyond the line of junction of the two, and be able to commence their separation from this point; but, should the cyst have been opened in the line of adhesion between the cyst-wall and the peritoneum, it will be best first to remove its contents, as the

adhesions will ensure that no part of the contents can then get into the abdominal cavity. This should be carefully avoided, although Koeberlé thinks that in small quantity blood or cyst-contents in the abdominal cavity are not so dangerous as generally supposed. He employs chiefly napkins instead of sponges for the toilet of the peritoneum.

Professor von Nussbaum's remarks as to the danger of gut-ligatures slipping from the sunken pedicle are no doubt true; but I think I meet the difficulty by tying the arteries in the pedicle separately, as well as the pedicle itself, in cases where the vessels in the pedicle are unusually large, or by mishap I have divided the pedicle too close to the ligature. I think that instead of, or in addition to ligatures (of gut) round the whole or portions of the pedicle, it would be well, in cases of broad pedicle, to employ a strong darning-stitch ligature, knotting the ends securely. Like Professor von Nussbaum, I have freely used carbolic acid solution within the abdomen. In one case, with bad adhesions in the pelvis, I *poured* warm five per cent. solution into the abdomen, afterwards sponging it carefully out. The patient walked away from the hospital in three weeks, although one day her temperature sank to 94° Fahr. Oozing from the abdominal wall I have stopped more than once by free application of ice, without any bad effects. As to the posture after the operation, I think it is a mistake to keep the patient very flat on her back all along. The greatest relief is generally felt when she is raised up on several pillows; and flatus is often expelled from the stomach readily in this position which otherwise causes much trouble. The worst symptoms I have met with arise from vomiting; and I have not found any means of readily checking it, though I have tried most. Absolute starvation for twenty-four hours is a serious remedy for, mayhap, an emaciated patient. I think it best to give some nourishment, if necessary *per anum*, and I always give the patient a good meat breakfast on the morning of the operation. If five or six hours intervene before the operation, the patient will be in a fair way of getting the benefit of it; whereas if, as is commonly the case, the patient has nothing between her supper at 8 p.m. and the operation next day, except a cup of tea and dry toast, she must suffer from twenty-four or forty-eight hours' additional starvation, whether compulsory or unavoidable (from sickness).

I have always been surprised at the length of time during which many ovariologists allow the patient's bowels to continue unmoved. Any trouble I have ever had in this respect has arisen from following this practice; whereas, whenever my patient has had a motion, natural or induced, on the third or fourth day, I have seen good results. In several cases, I have found the urine alkaline on the fourth or fifth day, continuing

so for even ten days. Usually, acidity returns sooner, I have always washed out the bladder with a weak solution of carbolic acid after the second day if the alkalinity continue, employing a little apparatus. Nearly all ovariologists recommend the complete emptying of the sac before it is withdrawn from the abdomen. I think it will be found advantageous where there are adhesions only to draw off part of the contents, and then remove the trocar and tie the orifice, so as to prevent the escape of fluid. The sac is then to be by degrees withdrawn; and I feel convinced that there is much less difficulty in recognising the details of adhesions, especially if intestinal, when the sac is thus partially full of fluid than when it is completely empty and hanging in loose folds. When the sac-wall is very thin and weak, it will rarely be possible to empty it with a trocar; and I believe it will be found the best plan to incise it freely with a knife, and turn the patient on her side and let the contents run out. Much valuable time will thus be saved, and, in the end, with care, less fluid will get into the abdomen than in the ineffectual attempts to withdraw it by a trocar, while the cyst-wall is tearing continually, and each attempt to attach it to the trocar only leads to additional tearing.

It cannot be so important to include the peritoneum in the abdominal sutures as Mr. Wells supposes, seeing that Kœberlé and other successful ovariologists have never done so; and it must generally happen, when it is so included, that it will double up into the wound more or less, and prevent union throughout the entire thickness of the sides of the wound. Should it have been by mistake partially detached, it must of course be included. In a recent case, I united the muscular layers of the opposite sides of the incision by gut-sutures, only including these layers, using in addition only superficial gut-sutures, the former being thus left completely sunken in the wound. The case did well; but the gut did not become metamorphosed as usual, being of bad quality. This mode of closure, however, seemed to me very satisfactory, securing complete adaptation of parts, and avoiding the peritoneum.

I am sorry to say the antiseptic method is not popular in practice, so far as my experience goes, with the profession. No doubt there are few who have not used remedies which contain carbolic acid, and there are many who profess to employ "the antiseptic method" who do nothing more. But, unfortunately, using the five or the three per cent. solution is not antiseptic treatment; and to get together half-a-dozen gentlemen acquainted with the method in its details and willing to take the trouble of carrying them out minutely is, in the provinces, not an easy thing. Undoubtedly, though the trouble of any operation treated antiseptically may be somewhat greater, the total trouble

with the whole case will be infinitely less, as well as more satisfactory. Mr. Knowsley Thornton's late splendid successes with this method in ovariectomy, as well as Professor von Nussbaum's, speak strongly for it in this serious operation. Some time ago, I was challenged at a medical meeting, where I related a case of ovariectomy which I had treated antiseptically, and I was asked why I was not content to follow the path Mr. Wells has so successfully trodden. Mr. Thornton's successes, under Mr. Wells's own eye, are a good justification, if such were needed.—*British Medical Journal*, Nov. 30, 1878, p. 792.

88.—ON CIRCULAR CONSTRICTION IN THE EXTRA-PERITONEAL TREATMENT OF THE PEDICLE IN OVARIOTOMY.

By LAWSON TAIT, Esq., Surgeon to the Birmingham Hospital for Women, &c.

After a very considerable experience of various methods of dealing with the pedicle, I feel disposed to think that, save in those cases where it is exceptionally narrow or short, the most satisfactory results will be obtained from the extra-peritoneal method of dealing with it. But whether that be so or not, there unquestionably will always remain a large number of cases in which it will be altogether impossible to drop the pedicle back into the abdomen, whether it has been treated by cautery or ligature; and along with these must be classed cases of removal of uterine tumours, in very few of which could either cautery or ligature be trusted to.

It comes to be therefore a very important matter to have a clamp which will deal effectually with a thick pedicle, and in such a way as to allow the abdomen to be closed as effectually as when the pedicle is narrow. It has been my misfortune to lose two cases of hysterotomy because I had not succeeded in accurately closing the wound, and this has also, I fear, occurred to me in more than one ovariectomy where the pedicle was very thick.

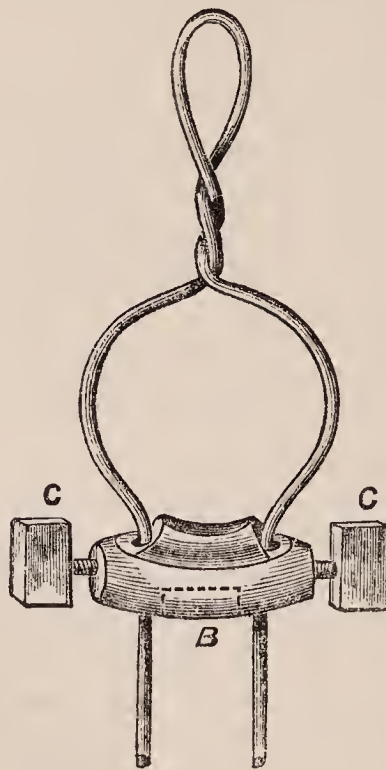
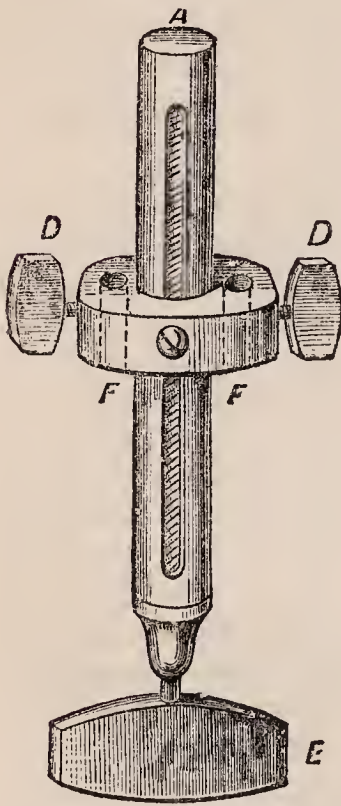
In a recent case of hysterotomy I used two of M. Pean's wire clamps, dividing a very thick pedicle between them. The patient, already very anæmic, succumbed to recurrent hemorrhage, and on post-mortem examination it was found that one of the wires had broken by over-strain, and that the other had loosened by shrinking of the pedicle, or by having cut its way through the tissues. In any case the instrument seemed faulty in that its wires were so thin that they either broke or cut, and these are of course both dangers which it is desirable to obviate.

The object to be obtained was evidently a force of circular

constriction of the pedicle by some agent which would not cut and which could be depended upon not to give way under any strain which would be likely to be applied to it. The circular method of constriction is imperative, for that is the only way in which a thick pedicle can be arranged so that the wound can be accurately closed round it.

After a large number of experiments I selected thick copper wire (No. 12), nicked, and completely softened by being made red-hot and allowed slowly to cool, as being the best material to work with.

For use I bend it into a loop, as seen in the right-hand figure,



ready for the operation. After it has been placed round the pedicle, the collar B is run on close up to the pedicle. The handle A E is then also run on, the ends of the wires running with the holes F F, and the end A fitting into a counter - sunk hole shown by a dotted line at B. The pinch-screws D D are then closed tightly down on the wires, the screws

c c being quite loose. The handle at E is then turned slowly till the loop of wire firmly constricts the pedicle. The tumour is then removed, and if there be any bleeding, a few more turns of the handle E will secure it. When this is done, the pinch-screws C C are to be securely tightened down on the wire, and those at D D loosened. The handle will then come off, leaving the wire clamp with its collar, as seen in the right-hand figure. The ends of the wire are then to be turned slightly up, and the wound closed and dressed as usual.

I have, within the last few weeks, used this clamp in thick pedicles in three cases, with perfect success, and one of those was the pedicle of a uterine myoma.—*Obstetrical Journal*, April 1879, p. 19.

89.—ON INDURATIONS OF THE BREAST BECOMING CANCEROUS; THEIR DIAGNOSIS AND TREATMENT.

By Sir JAMES PAGET, Bart., F.R.S., &c.

In a paper published in the tenth volume of the Reports (1874), I pointed out the frequency with which portions of mammary gland become cancerous after long-enduring eczema or other eruption on the nipple and areola. My observations were wholly clinical; and for a partial explanation I could only suggest that which Mr. Butlin has since proved. In papers in the Medico-Chirurgical Transactions (vols. lix., lx.) he has shown that structural changes may be traced extending from the diseased part of the skin along the epithelial linings of the gland-ducts in the nipple, and thence along their branches into the acini of the cancerous part of the gland. These "become dilated and filled with proliferating epithelium, which is at length, so to speak, discharged into the surrounding tissues. . . . The carcinoma thus formed is, therefore, essentially a disease of epithelium."

The cases of cancer thus following eczema are illustrations of a general rule, that a part which has long been the seat of constant or often-recurrent inflammation, or, if I may write with intentional obscurity, of frequent or constant "irritation," is apt to become cancerous. Similar instances of the rule are observed in tongues long affected with psoriasis or ichthyosis, in uteri long or often ulcerated, in scars that often "break out," in lower lips long cracked or excoriated, in warts often irritated, sore, and scabbed, sometimes in old scrofulous or other ulcers, or in sinuses. But in all these and other like cases we may observe that irritation alone, or under all conditions, is not sufficient to induce cancer. The change rarely, if ever, happens unless in parts which are apt to become cancerous, even without evidence of previous morbid change, and unless at the time of life at which the part is, as if of itself, chiefly liable to cancer; and it is the more likely to occur the stronger the inherited disposition to cancer. It may, therefore, be deemed very probable that the chief or sole effect of "irritation" is, by inducing a degeneration, to render the parts more fit for the invasion of a disease which is essentially of internal origin.

A chief interest in all these and the like cases is that they give good opportunities for the study of the process of "becoming cancerous," a process of the highest importance in both pathology and practice. Some of the structural changes ensuing in it are known; but the visible changes do not explain the much greater changes in the method of life, and in the influence, of parts which have become cancerous. As we watch an inflamed or irritated part in any of the cases I have cited, we

can be sure that there is some long time in which it is not cancerous; and that if it can now be cured or removed, the probability of the patient having cancer is annulled, or greatly diminished or deferred. But we can be nearly as sure that if the irritation continues cancer will appear, and that when this change shall have ensued we shall be powerless to reverse its course. The change is as great as that engendered by the fertilising of a spore. It would be hard to name a morbid process more worthy of study from every point of view.

I cannot hope that the facts which I have now to tell will be directly useful for their study. If they be useful for it, it can only be by provoking such researches as Mr. Butlin's. But though perhaps useless for exact pathology, I will yet hope that they may teach something for surgical practice.

Among the occasional difficulties of diagnosis of the scirrhus cancers of the breast, one of the most frequent is that in which a portion of the mammary gland appears to be merely hardened, as if by some slow inflammatory or cirrhotic change.

Of many instances of this kind that I can remember, some have got well; the hardening has disappeared, and the breast has again become healthy, and remained so; but some have been or have become cancerous. I wish that I could give such sure guidance to signs for diagnosis in these cases as would tell always how the better may be distinguished from the worse, but I can only indicate the characters that may generally be relied on.

The history of the cases rarely helps in diagnosis. Whether cancerous or not, the induration may have been found "by chance," no pain or discomfort having preceded; it is often observed, scarcely changing, for many weeks or months. It may have followed injury or have seemed spontaneous; and similarly, like cancer, it is not, unless by accident, associated with fever, or any defect or disturbance of general health. What the patient feels is as little decisive. Commonly scirrhus cancer, in its early stages, is painless, and so are the simple indurations; but in either case the fear of cancer, and the watching for its supposed characteristic pains, is very likely, in some persons, to produce subjective imitations of them.

Signs more to be relied on are that the indurations least likely, or not likely, to become cancerous are indicated by degrees of hardness less than those of scirrhus cancers of the same size and observed duration. They are more tough than hard; commonly, also, less rounded, less nodular, and less well defined. I say, "they," but I have never seen in a breast more than one induration at all resembling cancer. If there be many, they are not likely to be or to become cancerous. More-

over, mere indurations do not involve the skin, do not invade or infiltrate it, or produce in it any puckering or dimpling, as by drawing a part of it towards their own mass. In this, indeed, I think there may be an almost unfailing diagnostic sign. When the nipple or any part of the skin of a breast is drawn-in to a subjacent hardness, this is almost certainly cancerous. It may have been so always, or it may lately have become so; but there are few diagnostic signs more sure, even though the retraction or dimpling may not be visible unless in certain positions of the breast or certain methods of holding it. Such dimpling may, in very rare instances, appear when an induration is suppurating at its centre, but I do not know any other condition in which the retraction over cancers is imitated; and in this case the diagnosis may rest on the fact that the retraction over cancer takes place towards a part of the induration which is harder or not less hard than the rest, while that over suppuration is towards a part softer or much more elastic than the rest. Softening, or a feeling of elasticity, at or near the centre of an induration may always be regarded as a favourable sign, but the observer must take care lest he mistake for it the softness of a thick layer of subcutaneous fat over a deep-seated cancer. Such softness or elasticity probably indicates either suppuration such as may happen in a chronic abscess, or else the gradual increase of fluid in a cyst formed by dilatation of a gland-duct within a toughened portion of the gland. This character should be carefully searched for. Few diagnoses in surgery are more difficult than that between some serous cysts and some cancers in the breast, and the sign mainly to be relied on is the presence or the absence of elastic tension on firm, deep pressure over the middle of the "lump."

This same feeling of elastic tension may be the chief sign for diagnosis between cysts surrounded by tough gland substance, and another disease, which may also closely resemble both scirrhus cancer and the probably cirrhotic induration of part of the gland, of which, chiefly, I am writing—namely, a mammary glandular tumour (adenoma) of unusual hardness. The uniform hardness of such tumours may distinguish them from cysts; and from both cancers and simple or cirrhotic indurations they may generally be known by their giving a sensation when we press them alternately on one border and another, as if they moved *in* the surrounding gland-substance, not *with* it. The difference is difficult to describe, sometimes difficult to perceive; but it is a real one, due, probably, to these tumours, however hard, being encapsuled, and not, as are cancers and indurations, continuous with the surrounding substance of the gland.

If now, with such signs as I have indicated, a diagnosis may

be made that an undurated portion of a mammary gland is not cancerous, yet it must always be borne in mind that it may be becoming so; that changes may be taking place in it, such as those which Mr Butlin has traced in the eczematous breasts; and that we do not know the time in which the changes may become complete and irremediable. A sure means of timely cure would be of huge value. I cannot pretend to know one; yet I can hardly doubt that cure may sometimes be effected with liquor potassæ alone or with iodide of potassium.

The plan which I have usually followed is to direct that the hardened and adjacent parts of the breast should be covered with belladonna plaster, and that the patient should take a drachm of liquor potassæ and two grains of iodide of potassium three times a day after meals, in not less than four ounces of any simple liquid. (Nothing destroys the nasty taste of the potash better than a little liquorice.)

I do not suppose that the belladonna plaster has any direct medicinal value; but it may alleviate pain, and it has the great advantage of saving the diseased part from being constantly touched or handled. Of the curative influence of the liquor potassæ I think there is sufficient evidence. I have seen so many instances of induration of the mammary gland disappearing during its use, that, after allowing all that can reasonably be claimed for the belief that they might have disappeared even though no treatment had been used, there would remain facts enough to justify the belief in its efficacy. And this gains some confirmation from the reputation which the liquor potassæ once enjoyed for curing cancer. I have given it very often, and never saw reason to believe that it has any other influence on cancer of the breast than that of amending some of the conditions, such as gout or the lithic acid diathesis, with which the cancer being combined may be made more acute or more painful. I believe that the cures, if any, which it effected were those of indurations of the breast such as I have described, and which were mistaken for cancer.

Hard mammary glandular tumours, such as I have referred to, should be removed without delay. I think they are likely to become cancerous, though this change very rarely if ever happens in the common and softer form of the same tumour. And it is well, in operating, when the patient is insensible, to cut freely into the tumour. If it can be easily and completely enucleated, there is no need to remove any of the surrounding substance of the gland. If it seem continuous with this substance, a very wide excision is advisable, or the removal of the whole breast.

For the indurations which the liquor potassæ fails to cure, a similar course may be right. It may seem an unreasonable

thing to perform a serious operation for a disease which may never, or only at some uncertain time, become cancerous. Yet I cannot doubt that the operation should be advised for such an induration of the breast as I have described if there be great difficulty of diagnosis between it and scirrhus cancer, if the patient be of an age at which cancer is likely to occur, and if the induration have not been evidently reduced in size by treatment continued through a month or six weeks. And the manner and extent of the operation should be determined by the appearances found either by puncture with a fine trocar or by a free and deep incision into the hard mass.—*St. Bartholomew's Hospital Reports*, vol. xiv., 1878, p. 65.

90.—SOME INCIDENTAL BENEFITS OF ANTISEPTIC SURGERY.

By Dr. ROBERT BARNES, Obstetric Physician to, and Lecturer on Obstetrics, St. George's Hospital, London.

There are virtues attending antiseptic surgery even beyond that of enhancing the patient's prospect of recovery from severe operations. The surgeon secures himself, as well as his patient, from the dangers of blood-poisoning. Had Maurice Colles performed the operation on the cancerous jaw, which cost him his life, under a cloud of carbolic spray, is it not reasonable to suppose that this admirable surgeon would have been safe from infection?

Sidney Smith used to say that doctors encountered the perils of pus and miasm with an heroism akin to that which animates the soldier in battle. But it is not the part of a good general to throw away needlessly a soldier's life. Nor can it be for the public good that a surgeon should, in his endeavour to save the lives of others, heedlessly risk his own.

Again: the still greater danger attending the performance of post mortem examinations may be minimised, if not annihilated, by the use of the carbolic spray. This, I believe, will be found far more effectual than the use of the film-gloves. This point is especially important. The performance of post mortem examinations is often an unavoidable public duty, as well as a matter of scientific interest. I have known not a few instances in which puerperal fever and other diseases sprang up in the track of the surgeon who had been engaged in this duty. In this, as in many other ways, the welfare of the surgeon is intimately linked with the welfare of his patients and of the public.

Thus, when we have realised the great facts: 1. That the field of surgery is enlarged by making operations, hitherto condemned, possible and safe: 2. That the success of many recognised operations is greatly increased—we have far from ex-

hausted the beneficial applications and consequences of Lister's great discovery.

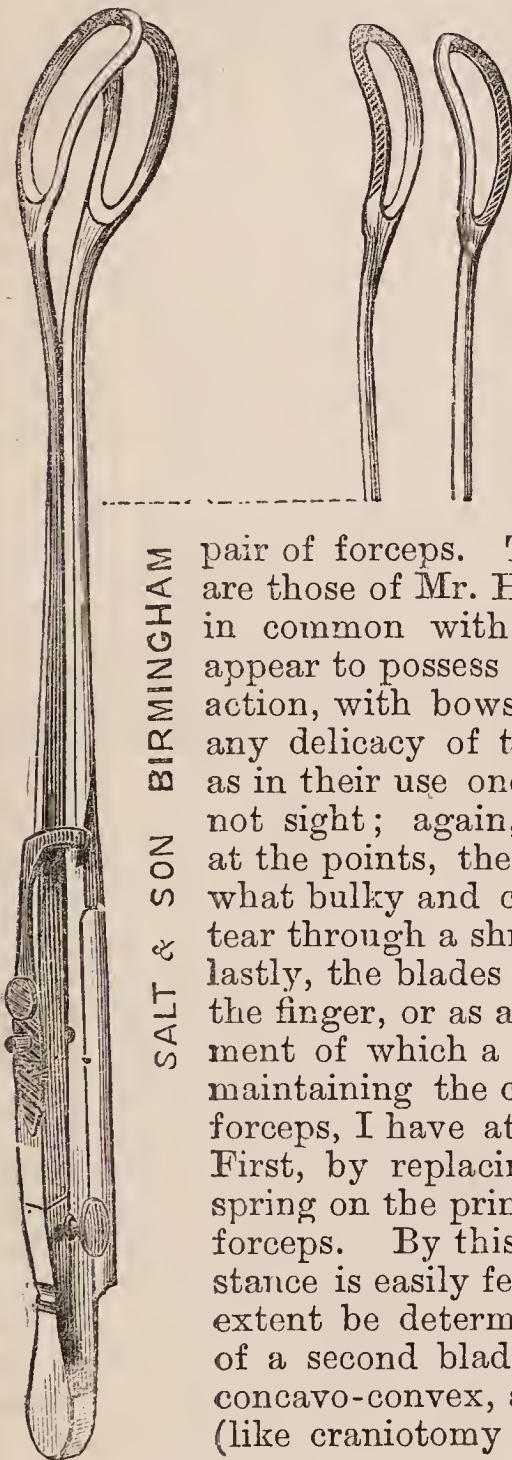
The question of the comparative dangers attending the major operations in our large hospitals, and in small special hospitals and private practice, is apparently solved. The special dangers supposed to be inherent in large hospitals are eliminated, and their many natural advantages remain untouched. It is now a matter of experience that ovariotomy especially is, under the antiseptic method, as successful in our general hospitals as in small institutions, or in private practice.

Might not the antiseptic spray be more widely extended to the practice of medicine, and especially for the prevention of the spreading of infection? We are familiar with various vaporisers for diffusing pleasant perfumes and vapours for inhalation. Some put their trust in saucers of Condyl's fluid. This might be justified if the contagium, emanating from a case of small-pox or scarlatina, could only be induced to deposit itself in the saucers and go nowhere else. But it is not so accommodating. The subtle particles diffuse themselves throughout the air, enter every cranny, cling to walls, floors, furniture, bedding, the clothes and persons of attendants. A disinfectant, to be effective and trustworthy, must chase the poisonous particles as fast as they start from their source. The true disinfectant is, therefore, volatile. Chlorine and sulphurous acid gas are excellent in this respect; but, if generated in the ordinary way, they are irritating to the lungs, and it is difficult to keep up a supply of uniform strength. A plan that promises better would be to use a vaporiser worked by a spirit-lamp, charged with a solution of one part of carbolic acid in a hundred, and to keep up a constant cloud of steam playing over the bed. The contagium would thus be continually met at its source, and the vapour would spread throughout the room. Having often operated with a stream of one in forty playing under my nose, I can affirm that there is no reason to apprehend annoyance from this plan. But the ventilation of most of our bed-rooms, which sooner or later become our sick-rooms, is but ill adapted to counteract infection. Every bed-room should be provided with an Arnott's chimney-valve to carry off the foul air; and, to maintain a proper current of clean air to wash out the room, it is essential to supply a stream from without. The best contrivance I am acquainted with is Tobin's tube. It acts better than an open window. When there is a fire in the grate, the combined action of the Tobin's tubes and Arnott's valves is very efficient. The doors and windows may be kept closed, and the ventilation goes on perfectly. If we were to lay over the mouth of the Tobin's tube, suspended on wire-frame, a sheet of gauze saturated with car-

bolic solution, the fresh air would enter charged with the disinfectant, and add to the efficacy of the spray-producers.

Surely, if septicæmia can be stamped out from the surgeons' wards, small-pox, scarlatina, and diphtheria should not be allowed to spread beyond the sick-room.—*British Medical Journal*, Jan. 11, 1879, p. 39.

91.—NEW OVUM AND PLACENTA FORCEPS.



This instrument was made for Dr. Hickinbotham, by Messrs. Salt and Son, Birmingham. Dr. Hickinbotham says: "Although the finger is the best possible instrument for the removal of an early ovum, a bit of placenta, or a shred of membrane from the uterus, every practitioner will admit that there are cases in which the finger requires either to be supplemented (as by a blade to act in apposition with it) or superadded, as by a

pair of forceps. The best forceps which I have seen are those of Mr. H. Morgan, of Lichfield, but they, in common with all ovum or placenta forceps, appear to possess certain defects. Thus, the cross-action, with bows for fingers and thumb, prevents any delicacy of touch, which is most important, as in their use one has to rely solely on feeling and not sight; again, as the blades approximate only at the points, they are only applicable to a somewhat bulky and consistent mass, and would easily tear through a shred of membrane or the like; and, lastly, the blades cannot be separated for use with the finger, or as a spoon or scoop. In the instrument of which a representation is annexed, while maintaining the curve and fenestræ of Morgan's forceps, I have attempted to remedy these defects. First, by replacing the scissor-action by a weak spring on the principle of the artery or dissecting forceps. By this arrangement the smallest substance is easily felt, and its nature can to a great extent be determined. Secondly, by the addition of a second blade which, with its fellow, forms a concavo-convex, and bites along its whole length (like craniotomy forceps), a shred of membrane or

a friable bit of placenta is grasped along a much larger surface, and thus the danger of tearing off is reduced to a minimum. By a catch, the blades are held together if it is desirable to use torsion. This arrangement also permits the instrument to be used without risk through a much more contracted os-uteri than would be possible with the ordinary forceps. Lastly, either of the blades can be used alone or with the finger. My best thanks are due to Messrs. Salt and Son, of Birmingham, for the skill and care which they have displayed in carrying out my ideas, and for the valuable practical suggestions by which the instrument has been most materially improved."—*Communicated*.

ADDENDA.

92.—ON THE ACTION OF ANÆSTHETICS.

[The following is extracted from the "Preliminary Report on the Action of Anæsthetics" presented to the Scientific Grants Committee of the British Medical Association by a Committee consisting of JOSEPH COATS, M.D., Pathologist to the Western Infirmary, Glasgow; WILLIAM RAMSAY, Ph.D., Assistant to the Professor of Chemistry; and JOHN G. MCKENDRICK, M.D., Professor of Physiology in the University of Glasgow.]

The present is intended merely as a provisional report, and no attempt will be made to give details of observations.

Two lines of inquiry soon opened themselves to us: first, to discover wherein the special dangers of chloroform consist; and, second, to try if some anæsthetic agent could be found which would avoid these dangers. We have also kept in view the investigation of the physiological action of anæsthetics in general, and the collection of evidence from the profession regarding the value and dangers of the anæsthetics at present in use.

In the first of these lines of inquiry, the much vexed question of the effects of chloroform on the respiration and the heart presented itself. Without going into detail, we may say that it soon became apparent to us that chloroform administered to dogs and rabbits has a disastrous effect on the respiratory centres; it is easy to kill one of these animals by pushing the chloroform till respiration is paralysed. In observing the state of the heart during these experiments, it could often be determined by auscultation that its contractions were maintained after respiration had ceased. It was apparent, however, that even when failure of respiration was more directly the cause of death, the heart was to some extent simultaneously affected; and there were even cases in which the heart appeared to fail at least as soon as, if not before, the breathing. Considering these facts, and bearing in mind that failure of the heart is often asserted in the reports of death from chloroform, we set about devising a method of experimentation by which respiration would be eliminated, and the effects of chloroform on the heart observed apart from that complication.

In the frog, we have an animal in which the movements of respiration are not necessary to life, so far as the heart is con-

cerned, as that organ continues beating long after these movements have ceased. After exposing this animal to the vapour of chloroform under an inverted jar till it was anæsthetised, we exposed the heart by cutting into the thorax in the middle line. The animal being again replaced under the jar, it was found that the heart became rapidly weaker, till it ceased beating. A similar experiment with ether showed a very different result. The exposed heart continued vigorously beating for a considerable time—in fact, as long as the experiment was continued.

With a similar view, a method was devised for warm-blooded animals. Rabbits were first used, and afterwards dogs. The animal was first anæsthetised; then the trachea was opened, a tube introduced, and artificial respiration begun by means of a double-acting pump (one cylinder forcing air in and another sucking it out). By an arrangement of india-rubber tubes, chloroform or any other anæsthetic could be introduced in the circuit between the pump and the trachea. It is to be understood that the air passes into the animal's lungs saturated with the vapour of the substance used. After artificial respiration had been set going, the heart was exposed by an incision in the middle line, which was carried by a pair of blunt scissors or bone-forceps through the ensiform cartilage and lower part of the sternum. This was effected generally with no serious bleeding. It soon became apparent that when chloroform is given in this way there is at once a most serious effect on the heart; the right ventricle almost immediately begins to distend, and the heart presently stops with the right ventricle engorged with blood. The heart had often, in the case of rabbits, virtually come to a standstill within a minute of the introduction of chloroform by the method described. The contrast was most striking when ether was used instead of chloroform; the other steps in the experiment being the same. Ether may be given for an indefinite period without interfering with the heart. We kept artificial respiration with ether in the circuit for an hour, not including twenty minutes occupied in producing anæsthesia, and at the end of that time the exposed heart was beating as vigorously as at first.

It was obvious, therefore, that, apart altogether from the respiratory centres, chloroform has a disastrous effect on the heart, while ether has no such effect. While presenting in this respect an enormous advantage over chloroform, it was yet apparent that ether has some great disadvantages. The chief of these is the tardiness of its action. In comparative experiments with rabbits, in which the anæsthetics were given on a towel, it appeared that with chloroform complete anæsthesia was produced in about three minutes, while in the case of ether it took

fifteen to twenty minutes, although the cloth was kept saturated. It occurred to us, therefore, to endeavour to find an agent which should be as potent an anæsthetic as chloroform, yet affect the heart and respiration as little as ether.

In testing the various agents used, we employed the methods described above. We administered them to animals, and watched the effects on respiration. We used the method on frogs by which the effect on the heart could be observed; and, in the case of some of them, we performed the experiment on rabbits and dogs, using artificial respiration, and exposing the heart. It may here be remarked, that in these experiments the anæsthetics were given intentionally in large doses, because, if any substitute for chloroform is to be found, it must be one which may safely be given in exceptionally full doses.

[The following substances were administered:—Benzine, acetone, pyrrol, bichloride of methylene, amylene, butyl-chloride, ethene dichloride, methyl chloride, ethyl chloride, and nitrous ethyl ether.]

The above substances all present disadvantages which render them unsuitable for general use as anæsthetics. There remain two agents, the actions of which are more promising. These are isobutyl chloride and ethidene dichloride.

Isobutyl chloride (C_4H_9Cl).—1. *Experiments on the frog.* When it was administered under a glass jar, complete anæsthesia occurred in about five minutes. The heart was then exposed, and it was observed for thirty-five minutes, during which period its contractions were perfectly vigorous.—2. *Experiments on rabbits.* When it was administered with a cloth, anæsthesia was produced in three to five minutes. It was continued after anæsthesia for nearly half an hour without any interference with respiration.—3. *Experiments on dogs.* It was administered on cloth; anæsthesia was produced in four minutes. It was continued for half an hour, and respiration was unaffected, except slight occasional stertor.

Ethidene dichloride ($C_2H_4Cl_2$, an isomeride of ethene dichloride, produced from aldehyde).—1. *Experiment on frogs.* Administered as before. The exposed heart continued beating slowly but regularly throughout the experiment, which lasted in one case twenty minutes, and in another twenty-six minutes. Anæsthesia was produced in four or five minutes.—2. *Experiments on rabbits.* It was given on cloth as usual. Anæsthesia was produced within four minutes. On one occasion respiration stopped, but soon recommenced. In experiments with artificial respiration and exposure of heart, the cardiac contractions continued vigorous throughout, the observation being continued for forty minutes from the first administration.—3. *Experiments on dogs.* It was administered on cloth. Anæsthesia was pro-

duced in two to three minutes. In one case, anæsthesia was accompanied with some excitement manifested by squeaking; the animal was a young puppy. In another case, a large dog was kept fully anæsthetised for half an hour without the slightest failure of respiration or heart. The anæsthesia in this case was very rapid, and the administration was intentionally pushed with successive doses at short intervals, as evaporation took place. The recovery was rapid, and the animal manifested remarkably good spirits.

Two experiments were made on dogs, in which the heart was exposed, artificial respiration being kept up. No failure of the heart's action was observed, although the air passing into the lungs was saturated with the vapour of the substance. There was complete anæsthesia. On quickly removing the bottle containing ethidene dichloride and substituting chloroform, the right side of the heart began almost immediately to become distended and to be dark in colour, and the activity of the heart rapidly failed. The contrast between the effects of the two substances on the heart was most striking. Practically, a dog will live for a lengthened period in a state of complete anæsthesia under the influence of ethidene dichloride, whilst it will die in a short time when chloroform is used.—*British Medical Journal*, Jan. 4, 1879, p. 1.

93.—ON THE DICHLORIDE OF ETHIDENE AS AN ANÆSTHETIC.

Since the publication of the preliminary report (*vide* previous article), we have administered dichloride of ethidene to six patients in the Western Infirmary of Glasgow, and the results have been so satisfactory as to induce us to report them without delay. The cases were as follows.

Case 1.—A young man aged 18, under the care of Dr. Patterson, had an abscess opened near the hip-joint. The anæsthetic was given by pouring a small quantity on a piece of lint in a tumbler, and by holding the tumbler over the patient's nose and mouth so as to allow the entrance of air along with the anæsthetic into the respiratory passages: a method regarded by Dr. Patterson as both safe and economical. The anæsthetic was given at 9.19 a.m.; at 9.27, there was complete anæsthesia; the operation began at 9.29, and was over in a few minutes; it was certainly completed at 9.32, and the patient was waking up at 9.35. During the anæsthetic state, several sphygmographic tracings were taken by Dr. Samson Gemmell, which indicate a normal amount of arterial tension. Before the operation, the pulse was rapid, probably from excitement. During the operation, it became slower and fuller; but no

numerical observations were made, as our attention was concentrated on the general phenomena. The recovery was rapid, and there was no vomiting, sickness, or headache. About half an ounce of the substance was used.

Case 2.—A girl aged 13 was also under the care of Dr. Patterson, with abscesses in the thigh, from spinal disease. One abscess pointed towards the outer aspect of the thigh, while another was more in front. The opening of the first was the special operation at this time. The notes record that, before administration of the anæsthetic, the pulse was 134. At 9.47, the anæsthetic was given, and at 9.50 the pulse had fallen to 120; at 9.54, it was 100, and at 9.55 anæsthesia and muscular relaxation were complete. At 9.56½, there was vomiting (which was undoubtedly caused by the patient having had food in the morning, contrary to orders). The operation was over at 10.1, and at 10.3 the patient was recovering. At 10.5½, she was able to answer intelligently. The amount used was three drachms and a half.

Case 3.—A girl aged 8 was under the care of Dr. George Buchanan. The operation was removal of the forefinger for strumous disease of bone. At the beginning, the pulse was 120. The anæsthetic was given on a towel in the usual way of giving chloroform in Scotland; in one minute and a half, it had fallen to 110; in two and a half, to 105; in five, to 102; in six and a half, to 100; and in seven minutes anæsthesia was complete. The operation was begun when it was observed that the face had a healthy colour. At eight minutes and a half from the time the anæsthetic was given, the breathing became slightly stertorous. At the end of fourteen minutes, the operation was over, and the pulse was 105. Dr. Buchanan specially referred to the healthy appearance of the countenance. The patient recovered from the anæsthetic, so as to answer questions, in about two minutes, and there was no headache, sickness, or vomiting afterwards.

Case 4.—The same patient as in No. 2 had another large abscess (the one in front of the thigh previously alluded to) opened by Dr. Patterson. At 9.9 a.m., before the anæsthetic was given, the pulse was 140; at 9.10½, it had fallen to 110; at 9.12, it was at 104; at 9.13, there was slight flushing of the face; at 9.14, the pulse was 100, and there was slight stertor; and at 9.15 there was complete relaxation and absence of reflex movements. The operation was quickly over; at 9.22, the pulse was 110; at 9.23, consciousness had returned; and at 9.24, the pulse had risen to 130. The amount used was four drachms.

Case 5.—A very powerful young man, aged 21, had the operation for the radical cure of inguinal hernia performed by

Dr. George Buchanan. The Committee regard this case as specially interesting, on account of the length of time the patient was under the influence of the anæsthetic. Immediately before giving the anæsthetic, the pulse was very rapid from excitement—about 140. At 9.17, the anæsthetic was given; at 9.18½, the pulse had fallen to 110; at 9.22, pulse 110 and slight struggling; at 9.23, it was observed that the face was slightly flushed; at 9.24 and at 9.26, considerable struggling; at 9.28 (eleven minutes since the anæsthetic was given), the pulse had fallen to 74, and the operation was commenced; at 9.35, pulse 70, and respirations deep and regular; at 9.40, the operation was over, and at 9.42 consciousness returned. About 9.41, there were deep anæsthesia and complete muscular relaxation; a condition specially necessary for such an operation. Dr. Buchanan expressed himself to the students present as thoroughly satisfied with the result, and he specially alluded to the tranquillity of breathing and the healthy colour of the lips and of the countenance. The amount used was an ounce and a quarter.

Case 6.—A little girl aged 8, pale, emaciated, and excited, had several pieces of diseased bone removed from the ankle by Dr. Buchanan. Before giving the anæsthetic, the pulse was small, thready, and so rapid as to be uncountable. The anæsthetic was given at 10.3½; at 10.5½, the patient was “over,” and the operation was begun. It was completed in a few minutes, and, after 10.10, no anæsthetic was given. She quickly recovered consciousness, when there was some retching. It appears that this girl had been ailing with sickness and occasional retching for several days previously. The amount used was about two drachms. Dr. Finlayson, one of the physicians, happened to be present, and he directed attention to slight rigidity of the muscles on the right and left sides alternately during the operation. It is probable that such spasms, sometimes seen during the administration of chloroform, are reflex, due to nerve-irritation by the operation; but this is a point to which the Committee will direct further attention. In this case, the pulse remained feeble throughout, and the diminution in frequency, so marked in the other cases, could not be determined.

Such is a short record of the six cases observed by the Committee. Three features are of special interest. 1. There was no injurious effect observable on the respiratory mechanism, although in all cases the anæsthetic was given in such doses as to produce complete anæsthesia and muscular relaxation, and in one (No. 5) the patient was deeply under its influence for twenty-five minutes. The question, however, of the limit to which the agent may be pushed without injuring respiration is

of such immense practical importance as to induce the Committee to make a further investigation on this point. 2. The pulse diminished in frequency and increased in volume, and in the deepest anæsthesia it was steady, regular, full, and compressible. There was no indication of failure of cardiac action in any case: a result anticipated from what had been previously observed in animals. 3. There was never any pallor of the countenance or blueness of the lips, but, on the contrary, and even during the deepest anæsthesia, there was a healthy flush on the face and the lips were rosy-red. Taking into account the change in the character of the pulse and in the colour of the face, it would appear that, in anæsthesia from dichloride of ethidene, the blood still remains in a normal amount in the arterial and capillary systems, and does not tend to engorge the venous system and right side of the heart, as is apparently the physiological action of chloroform.

The total result appears to the Committee to be, that dichloride of ethidene, as an anæsthetic, presents all the advantages of the ether, without any of its disadvantages; and that the following opinion of Steffen, given in Binz's *Evidence of Therapeutics*, p. 39, is correct in most particulars: "It is said to have the following advantages over chloroform, which it resembles in its ultimate action, namely, a pleasanter smell, the power of producing narcosis more rapidly, as well as without excitement or vomiting, more rapid recovery without after-effects, and altogether less danger." In our experience, narcosis has not been produced more rapidly than with chloroform, but rapidity of narcosis depends very much on the mode of administration. If an anæsthetic be given cautiously and in small doses at first, the supervention of narcosis will be slow; but if it be given in a large and concentrated dose at first, the time no doubt may be shortened. Again, in one case (No. 5), there was excitement, and in two cases there was retching. In both the latter, however, the circumstances were peculiar; in one, the patient had previously taken a meal; and in the other, sickness and retching had occurred for several days, and the patient was in a very weakly condition.—*Brit. Med. Jour.*, Jan. 25, 1879, p. 108.

94.—ON ANÆSTHETICS.

By Dr. C. MEYMOTT TIDY, F.C.S., Lecturer on Chemistry and Forensic Medicine at the London Hospital.

[The following is an abstract of an address read before the Association of Surgeons practising Dental Surgery:]

First of all, I will say a few words on the anæsthetics in common use. I will not trouble you with discussing the pecu-

liarities of certain anæsthetics, which, whatever may be their value, have as yet, on a large scale at any rate, "not been proved." Some have spoken highly of bichloride of methylene, with which I have myself experimented largely on the lower animals. It is, according to Dr. Richardson, who first suggested its use as an anæsthetic, less dangerous than chloroform. The quantity of the bichloride of methylene required to effect complete anæsthesia is less than that of ether, but greater than chloroform. Mr. Morgan speaks of having administered it one thousand eight hundred times to persons of all ages, and for periods varying from a few minutes to three-quarters of an hour, without a single accident. He also regards it as safer than chloroform, and speaks of the rapidity with which it effects complete unconsciousness, as a rule, two minutes only being needed; the rapidity of recovery, from one to three minutes only being required for the anæsthesia to pass away; the rapidity with which dangerous symptoms (if they occur) subside; and, lastly, the rapidity with which consciousness may be abolished if it return during the operation—as the chief points in its favour. The cause of death from its administration is syncope, not coma; hence a bloodless condition of the lips—a point easy to be noticed—is the principal indication of danger. The three anæsthetics, however, around which interest chiefly centres, are chloroform, ether, and nitrous oxide. Each has its advantages and disadvantages. Let us consider them.

The advantages of *Chloroform* are, that in most cases its administration is agreeable to the patient; at any rate, the earlier symptoms induced by its inhalation are pleasant. I admit that there are exceptions to this rule; but even in the exceptional cases—that is, where disagreeable effects occur—these effects pass away before the patient becomes insensible. Again, the rapidity of its action, the complete insensibility produced, and, above all, the entire absence of excitement (the actual quiet, in fact) when the insensibility is complete, constitute very important advantages where the operation to be performed is a long one necessitating manipulative delicacy.

The disadvantage of chloroform is the risk attending its administration; and this is worth our consideration for a moment. Death from the inhalation of chloroform may result, and probably most often does result (probably, I say, because the subject is one in which there is much uncertainty), from gradual paralysis of the respiratory muscles from the effects of the chloroform on the respiratory centres. This result, however, takes place more or less gradually. We can watch the change of breathing from its comparatively calm state, gradually becoming more stertorous and shallow. We can also note the pulse, changing from the normal condition usual during the

exhibition of chloroform, becoming quick, with a marked loss of strength, and perhaps irregularity. We can note the pupil passing gradually from a state of extreme contraction, common to a patient under the influence of chloroform, to a state of extreme dilatation. But this is not all; for in such cases, if active means be taken, we may fairly hope that danger will be averted and the life be saved. There is another form of death from chloroform, of infinitely greater moment; for here there is no warning, the time between life and death being practically absent. The heart at one instant beats well, and the next moment stops; the respiration at one moment is quiet and deep, and the next ceases. We may suppose the cause of death in such cases to be cardiac syncope; and, as regards treatment, it avails but little. And here I must note that everything likely to increase the chances of cardiac syncope, or, to use a better phrase, the direct action of chloroform on the heart, should be carefully considered beforehand. No doubt the administration of chloroform in a sitting posture must increase the risk of danger from this cause. Hence the administration of chloroform in such position should, if possible, be avoided. There can, moreover, be no doubt that certain diseased states, and notably fatty disease of the heart and dilatation of the heart, increase the risk arising from chloroform-inhalation; and, although these diseased conditions are not to be regarded as absolute reasons for withholding the administration of chloroform, nevertheless they have to be taken into consideration, and necessitate increased care.

The advantage of *Ether* is, that, as an anæsthetic, all records show it to be safer than chloroform. And the reason of this seems to be that, whilst, like chloroform, ether may destroy life by arresting respiration—in other words, by paralysis of the muscles of respiration—a form of danger which, I said, is more or less amenable to treatment; unlike chloroform, it does not destroy life by its depressing action on the heart. This is a very important distinction, and it is upon this depends what most English authorities, and nearly every American authority, agree in considering its greater safety. With this opinion in favour of ether over chloroform I agree; and all the disadvantages of ether sink into absolute insignificance before this point of superiority.

For ether has its disadvantages. It is, no doubt, compared with chloroform, an unpleasant anæsthetic; that is, unpleasant to the patient in the earlier stages before insensibility sets in. This disadvantage, however, may be an advantage; for, whereas cases are recorded (and I know of more than one myself) where patients have, after chloroform has been once administered, taken it habitually, in order to induce these

pleasant symptoms, I have never known patients to take ether for a similar reason. Again, the length of time required to effect complete unconsciousness with ether is in some respect balanced by the rapidity with which consciousness returns. The time required to get the full effect of ether varies to a far greater extent with different people than in the case of chloroform; as a rule, it is twice, whilst it is often three times as long, the quantity requiring to be inhaled being always very much greater. There are two further disadvantages in administering ether to be noted, and they are serious in many of the special operations for which anæsthetics are required; first, the restless excitement that often results from its administration, very different to the extreme quiet of chloroform unconsciousness; and, secondly, the danger of bringing a light near to the mouth when ether-vapour has been, or is being inhaled, lest the vapour catch fire. The excitement interferes materially with delicate work; whilst the possibility of fire, which, however, may do no more harm than frighten all concerned, is likely somewhat to unnerve where nerve is specially needed.

As regards *Nitrous Oxide*, its advantages are manifest—viz., the facility with which it can be administered; the little preparation needed so far as the patient is concerned; the ease with which the gas can be prepared in a pure state; the rapidity of recovery after the operation is over; and, above all, its almost absolute safety, and the absence of any bad after-symptoms. These constitute a series of very great advantages, placing it above all other anæsthetics in cases where the anæsthesia produced by it is of a sufficiently permanent character to allow the operation required to be performed. For this is its only disadvantage—viz., that it is practically impossible to perform prolonged operations under its influence. Whether this difficulty may be eventually overcome I will not stay to inquire; but, for myself, I am far from believing the difficulty to be insuperable.

How can the risks attendant on the administration of anæsthetics be reduced to a minimum?

Choose in every case the safest anæsthetic, having regard to the needs of the operation to be performed. First on our list as a safe anæsthetic stands nitrous oxide. Let that be given, then, whenever practicable. And here let me remark that, addressing as I am a society of true dental surgeons (I mean *true*, in contradistinction to dental surgeons falsely so-called), I do not purpose drawing any absolute distinction between dental operations and any other surgical operations. The circumstance that the operations of dental surgery are conducted in the mouth has, no doubt, to be considered just as any other operation on the mouth would have to be considered. I am of opinion,

therefore, that the rules which should guide us in our surgical operations should guide us in all others in this particular. My own opinion, and, what is of more importance, the general opinion of those best able to judge, is that, for the operations of dental surgeons, nitrous oxide is, as a rule, sufficient—as a rule, but not always, a more prolonged anæsthesia being at times required; and for this purpose it is the general opinion—an opinion, let me say, with which, as the result of not a limited experience, I entirely coincide—that ether should be given in preference to chloroform as the safer anæsthetic. But here the question arises, Is the administration of chloroform ever justifiable? That there are certain operations, and more especially operations in the mouth, where very often considerable manipulative delicacy is required, which cannot be well performed under the anæsthesia of ether, is, I think, unquestionable. In some cases, the extreme restlessness and restiveness, and the large flow of saliva induced by inhaling ether, render such an operation, to say the least, exceedingly difficult. In such cases, I suppose chloroform may, indeed must, be employed. But such cases I believe to be exceedingly exceptional, and (especially considering the extra danger in dental operations of death from cardiac syncope, owing to the sitting posture necessary) its administration should not be undertaken until after ether has been tried and failed, and the balance of evils has been most carefully and intelligently reconsidered. I do not see, therefore, that we should be justified in saying that chloroform should never be used in dental operations any more than we could say that it is never to be used in any other operation; but I do hold most strongly that its use should be confined to the exceptional cases where ether has been tried and found not to succeed.—*British Medical Journal*, Jan. 4, 1879, p. 3.

95.—ON CODEIA AS A SEDATIVE.

By Dr. ROBERT SAUNDBY, Assistant-Physician to the General Hospital, Birmingham.

No symptom is more distressing to a patient than frequent coughing, and none demands more judicious treatment on the part of the practitioner, if he would avoid undoing with his cough-mixtures all the good he is attempting by his more general therapeutic measures. In phthisis, the presence of anorexia makes us unwilling to give opium or morphia, and frequently, when we do so, we have reason to regret it. Many patients, especially gouty subjects and those who suffer much from derangement of the liver, are intolerant of opium and morphia. On account of these difficulties, I have been led to employ codeia in such cases, in the hope that it might be of

service, and it has succeeded beyond my anticipations. In phthisis, it allays cough without disturbing the digestive system; and, in the other class of cases, I have found it tolerated when opium and morphia were not. As an instance of the latter, I may quote the case of a medical friend, a member of a gouty family, a frequent sufferer from migraine and derangement of the liver, and well aware of his intolerance of preparations of opium. He complained of a troublesome cough, depending on slight catarrh of the trachea and bronchi, and, at my suggestion, tried codeia, with all the benefit and none of the ill effects of opium. I prescribe the drug in doses of a grain, dissolved in syrup of tolu. The French medical papers constantly contain advertisements of codeia syrup, and probably it is well known as a cough tincture in this country; but I was not aware, and others may have been ignorant as I was, that it has those advantages over the preparations of opium and its other alkaloids. I therefore venture to call attention to it. Its value in diabetes is, of course, fully recognised.—*British Medical Journal*, April 12, 1879, p. 545.

96.—THE THERAPEUTICAL VALUE OF DRUG-SMOKING.

By Dr. REGINALD E. THOMPSON.

There are six channels by which drugs may be made to enter the system:—1. By the skin externally. 2. By the mouth into the stomach. 3. By the rectum into the bowels. 4. By subcutaneous injection under the skin. 5. By the mouth into the lungs. 6. By the blood-vessels. Of these six methods the introduction of drugs directly into the blood-vessels by means of a fine syringe is accompanied with so much danger as to preclude their use in this fashion for therapeutical purposes, and the effects of drugs thus given can only be studied in experiments on animals, and from those accidental cases, happily rare, in which a subcutaneous injection has been followed by such speedy and extraordinary symptoms as to lead to the conclusion that a small blood-vessel has been accidentally pierced and the injected fluid has thus found its way directly into the circulation.

If this be a just conclusion, then it follows that the less the tissue intervening between the channel of introduction and the blood-vessels, the more rapid will be the absorption, the more intense the effect, and consequently the smaller will be the requisite dose. Considering, then, the special arrangement of the blood-vessels in the lungs as so disposed that the interchange of gases should take place freely with as little let and hindrance as possible, it might be fairly conjectured that absorption through the air passages would more closely approx-

imate to the immediate introduction into the blood-vessels in rate of absorption and intensity of effect than any of the other modes of administration. The inhalation and the smoking of drugs—methods by which medicines may be delivered into the lungs—have, however, been much neglected, and left chiefly in the hands of irregular practitioners of the art of healing, who have taken their advantage to make free use of and to reap a pretty abundant harvest from these most potent methods of administering remedies. The few vapours that are on the list of the British Pharmacopœia are of modern date, and there is a total omission of any means for the pulmonary introduction of drugs by smoking—a form of therapeutics with which probably no practitioner would like to dispense when he has a case of asthma under treatment.

Asthmatic patients, consequently, have to resort to the various patent remedies which are sold for their use, and with which they are generally fully acquainted: the benefit they receive from them is often great and immediate, and the physician is therefore compelled to advise the continued use of secret remedies which are certainly valuable but very objectionable from the empirical nature of their value. Moreover, it is, I conceive, a reproach to scientific knowledge to be out-done and to rest satisfied with being beaten by any one outside the profession, and the reproach is the greater if the secret remedies be only, as seems to be probable, particular combinations (in a special form) of drugs, the use of which is already recognised by the profession. It seemed, therefore, advisable that a series of experiments should be made on the therapeutical value of drug-smoking, and the results that have been obtained are here offered as a preliminary contribution to this important department of therapeutics.

There are several ways in which medicines may be administered into the lungs; by inhalation with steam, as atomised fluids by insufflation; or by fumigation with powders, prepared so as to burn freely in the air; or lastly, by smoking.

A number of experiments were made with powdered leaves, obtained as fresh as possible, and soaked in nitre; and this method of administering remedies may be adopted in some cases with advantage; but for accurate results it is evident that some other method must be used, inasmuch as it is impossible to estimate the effective dose by such haphazard introduction.

The simplest and surest method for such a purpose is the use of paper soaked in a weak solution of nitre to make it burn continuously, and dipped afterwards in the tinctures or solutions of the drugs to be tested, the paper being rolled into cigarettes of uniform size.

In all these experiments I have received the valuable help of

Mr. Leach (the head of the dispensing department of the Brompton Hospital for Consumption), to whom my acknowledgments are due.

The first point to settle was the best paper for the required purpose, and after many trials it was found that the Swedish filtering paper, manufactured expressly for the use of chemists, and calculated to leave as little ash as possible after incineration, is the best, being absorbent, burning freely, and giving out but little odour of burnt paper.

In order to make the paper burn with certainty it has to be dipped in a weak solution of nitre, and to prevent too free combustion from the access of air, the cigarette should be rolled of a definite size, not more than a quarter inch in diameter, and the rolling should be as tight as possible.

The most rapid way of rolling is obtained from the use of a stick of the required size, slightly tapering, so that it can be easily removed when the cigarette is finished.

The odour of burnt paper is so disagreeable that it was necessary to disguise it, and it was found that the best addition for the purpose was tincture of tobacco, to which may be added a small quantity of oil of anise.

The following formula represents the basis used for each cigarette:—Swedish filtering paper, size, 4 in. \times $2\frac{1}{2}$ in. Potassæ nitratis, gr. $\frac{1}{4}$; tinct. tabaci, ℥ x.; olei. anisi, ℥ $\frac{1}{8}$. The tincture of tobacco was made with $2\frac{1}{2}$ oz. of the leaf to a pint of spirit.

A solution of any drug which is to be experimented on can then be prepared, and the paper having been floated through the solution in a flat dish, when dry can be cut into a certain size, and the dose thus accurately measured.

The tobacco and nitre had no appreciable physiological effect when smoked by healthy individuals, and their remedial effect on a case of asthma must be considered to be very small.

Opium was the first drug with which experiments were made, and we began by trying the $\frac{1}{8}$ th of a grain, but soon found that the effects produced by smoking this quantity were too intense, and finally we came to a conclusion that the $\frac{1}{64}$ th of a grain of the extract of opium was sufficient for an initial dose.

Cigarettes with this quantity of opium were smoked by myself and three other healthy men, and in a few minutes a decided effect of dizziness was produced. These were smoked in the ordinary way, the smoke being partly rejected, and a very small quantity of the drug remains in the lungs under these circumstances; if the full effect of the dose be desired, the smoker should be instructed to expand the lungs with full inspiration and retain the smoke in the lungs.

With ordinary smoking it is probable that not more than

half the quantity of opium in the cigarette could be counted as effective; so that from the above instances we may conclude that the $\frac{1}{128}$ th of a grain of extract of opium can produce an effect when administered into the lungs by smoking.

The following cases may be given as showing the effective action of this extremely small dose:—

Edith C., aged 22, suffering from phthisis of two years standing, with a constant and harassing night cough, which used to keep her awake. She had been having very little rest when she applied for advice. She was advised to smoke one of the above cigarettes at night. Only half a one was smoked at the time, and the report given at the next visit was that she had slept well from 10 p.m. to 9 a.m., and was drowsy when awakened.

Ralph W., aged 24, suffering from phthisis, and subject to night wakefulness. For the last three months he had never slept for more than two or three hours during the night. The use of the opium cigarettes in this case induced sleep for seven hours.

Albert W., aged 30. Chronic asthma of six years duration. Considerable relief experienced from smoking the cigarettes, and undisturbed sleep during the night.

Jane S., aged 30. For two years has suffered from cough, which for some time has been very bad, chiefly at night. Much relief from cigarettes. Cough no longer prevents sleep.

Harriet B., aged 31. Mitral obstruction for some years. Has been under my care since 1877. Complaining lately of harassing night cough, sleeping only two hours during the night. Much relief from cigarettes; sleeping all night.

Thomas T., aged 49. Has been subject to empyoema and asthma for the last seven years. For the last week has been subject to attacks of asthma preventing sleep. Ordered to smoke the cigarettes. No spasmodic attack since using the opium cigarettes. Sleeps now from 9 p.m. to 6 a.m.

John B., aged 40. Asthma for the last two months. Cough and spasmodic attacks have been keeping him awake through the night. The cigarettes produced eight to nine hours' sleep, and prevented the asthmatic spasm.

Eliza S., aged 45. Suffering from syphilitic pulmonary disease, with constant cough at night. The use of the cigarettes stopped the cough, and procured good sleep at night.

Eleanor W., aged 24. Suffering from bronchitis with destruction of lung. Very tight-chested at night, and kept awake at night. Relief and sleep from the use of the cigarettes.

The dose in the case of one healthy man was increased to $\frac{1}{32}$ of a grain of the extract; but this, together with the same dose of stramonium, caused so much and such prolonged

dizziness that such a quantity would be too large for ordinary cases.

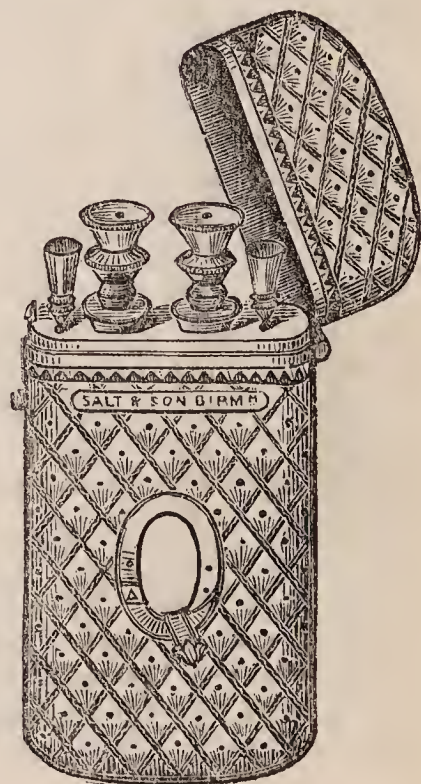
That many hours of sleep should have been procured, as in the first instance given above, by so small a dose as the two hundredth part of a grain of the extract of opium is rather startling, and far surpasses the results obtained from the subcutaneous injection, a mode of administering drugs which has hitherto been looked upon as likely to give the most concentrated results.

The smoking of opium is especially adapted for cases of harassing cough; the topical effect of the drug is immediately obtained without any part of the dose being wasted on other tissues; moreover, this mode avoids those objectionable effects which are a bar to the use of the drug when it has to be given by the mouth into the stomach.

Opium-smoking is particularly useful in those cases of laryngeal ulceration in which all attempts at deglutition are accompanied with extreme pain, and the topical effect of the anodyne chiefly sought. It is also useful in those undeveloped forms of asthma which I have already described in a previous number of this journal, as characterised by night cough; and lastly, opium in this form is very serviceable in the dyspnoea of asthma, preventing the night attack by inducing sleep.—*Practitioner*, April, 1879, p. 267.

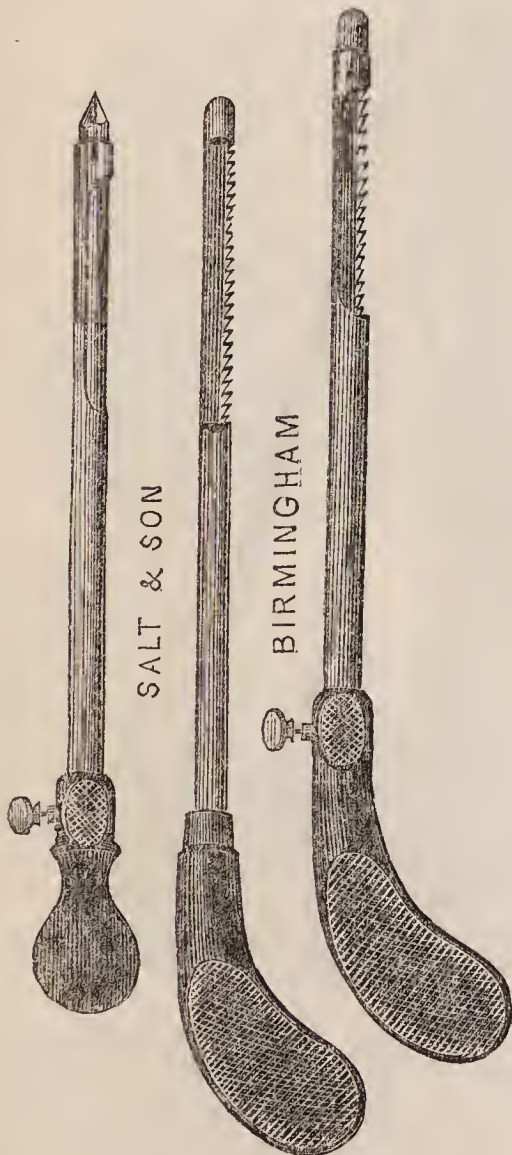
97.—NEW MINIATURE SUBCUTANEOUS SYRINGE.

This syringe, of which we annex an illustration, drawn to a scale of two-third size, will be found extremely useful and and convenient, being especially made for the waistcoat-pocket by Messrs. Salt and Son, Birmingham. The case is made of aluminium, and opens with a spring. The contents are silver syringe, two needles, and morphia bottle; it is extremely neat, compact, and handy.



98.—ON A NEW SAW AND KNIFE FOR SUBCUTANEOUS OSTEOTOMY.

Messrs. Salt & Son write: This instrument (of which we give an engraving) has been furnished to us by our New York correspondent, and appears so useful an invention that you may be pleased to find a place for it in *The Retrospect*. As will be seen by the engraving, it consists of a Trocar or fenestrated Canula, and a staff with blunt extremity; a portion of this staff at a short distance from the end is flattened, one edge forming a saw the other a knife. The instrument with its canula is first introduced and the trocar withdrawn, then the staff (or cutting instrument) is passed down the canula, either the saw or knife edge appearing through the fenestrum of the canula as may for the moment be required. The saw or knife can be used within the canula by a piston-like movement, or if more convenient, the instrument can be secured to the canula by a thumb-screw which is provided for that purpose.



The portion of the canula at the back of the fenestrum is made extra strong, and is of the same thickness as the blade, so that in sawing, there is no stoppage to the passage of the saw through any thickness of bone. The saw blade and knife are blunt at the extremity and guarded on all sides except on the limited cutting surface, so that the soft tissues are protected from injury. All that is necessary in using this instrument is to thrust the trocar and canula into the limb, the fenestrum of the canula being alongside of the bone, upon which the operation is to be performed.—*Communicated*.

99.—IMPROVED TRIPOD AND PULLEYS FOR TREATING SPINAL CURVATURE BY SAYRE'S METHOD.

This apparatus, suggested by Mr. Sampson Gamgee, and manufactured by Messrs. Salt and Son, Birmingham, possesses many advantages over those in general use, the principal one



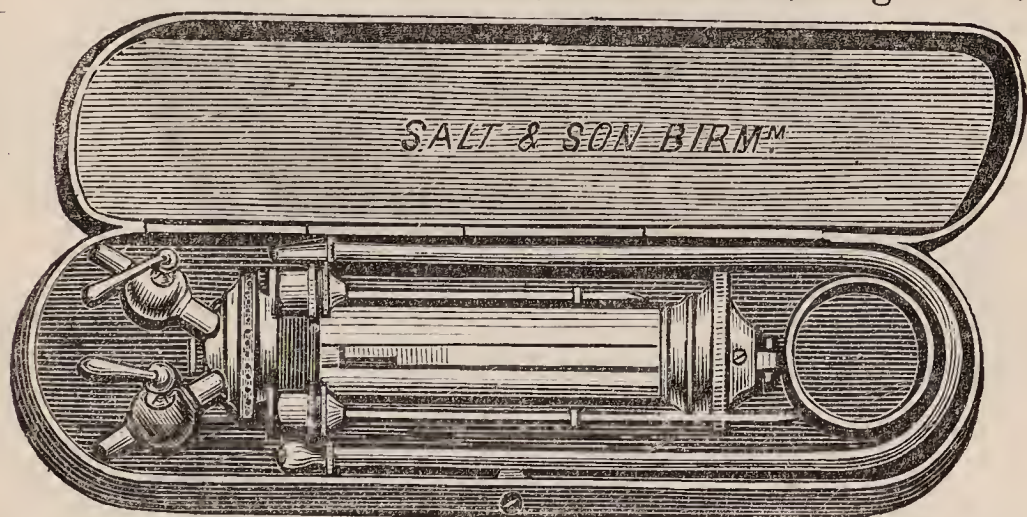
being that the pulleys are self-sustaining, and thus enable the operator to dispense with the services of an assistant. Such is the construction of this modified tripod that when the patient is raised, a very slight deflection of the cord to the right or left causes the pulley to become instantly self-retentive; that is to say, the body, whatever its weight, cannot possibly descend, al-

though the force which raised it be suddenly withdrawn. But immediate release is possible at any moment. A pull in a downward direction instantly releases the catch and allows the patient to descend without jerk. A further improvement in the apparatus consists in the whole of the parts being made easily portable.

The necessary jackets, bandages, plaster, shears, tripod, tin strips, and well-padded axilla and chin supports are contained in a box which can be conveniently carried in a brougham.

100.—NEW MINIATURE ASPIRATOR FOR FACIAL AND SUPERFICIAL ABSCESSSES.

This instrument, manufactured by Messrs. Salt and Son, Birmingham, is thus described by Mr. Bartleet, Surgeon to the

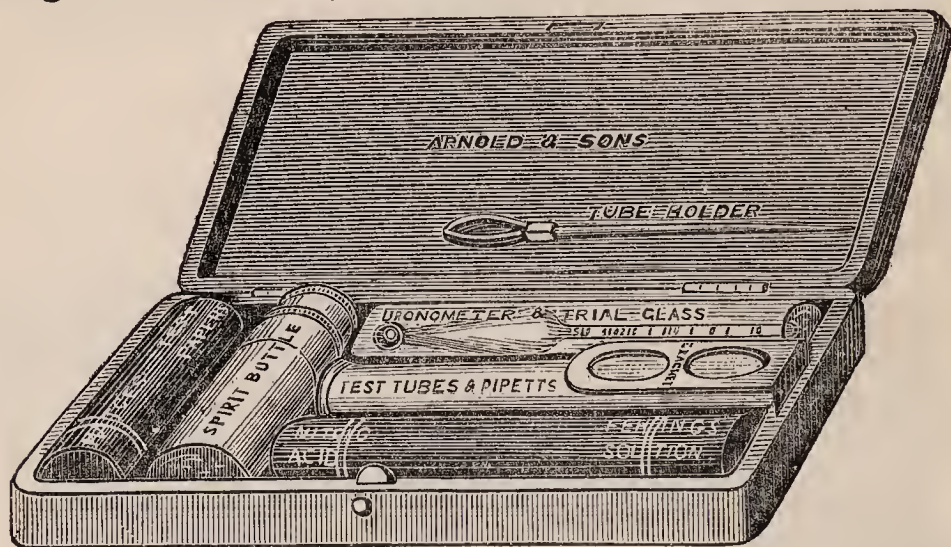


General Hospital, for whom it was designed:—"I have long wanted a small aspirator for opening small abscesses in situations where it is important to avoid a scar, and I am sure the one made for me by Messrs. Salt and Son, Birmingham, will be found convenient; firstly, in cases when the aspirator is used as an instrument of diagnosis; and, secondly, in the evacuation of small abscesses, such as those so often occurring on the cervical glands of delicate children. By this plan of treatment unsightly scars can usually be entirely prevented."

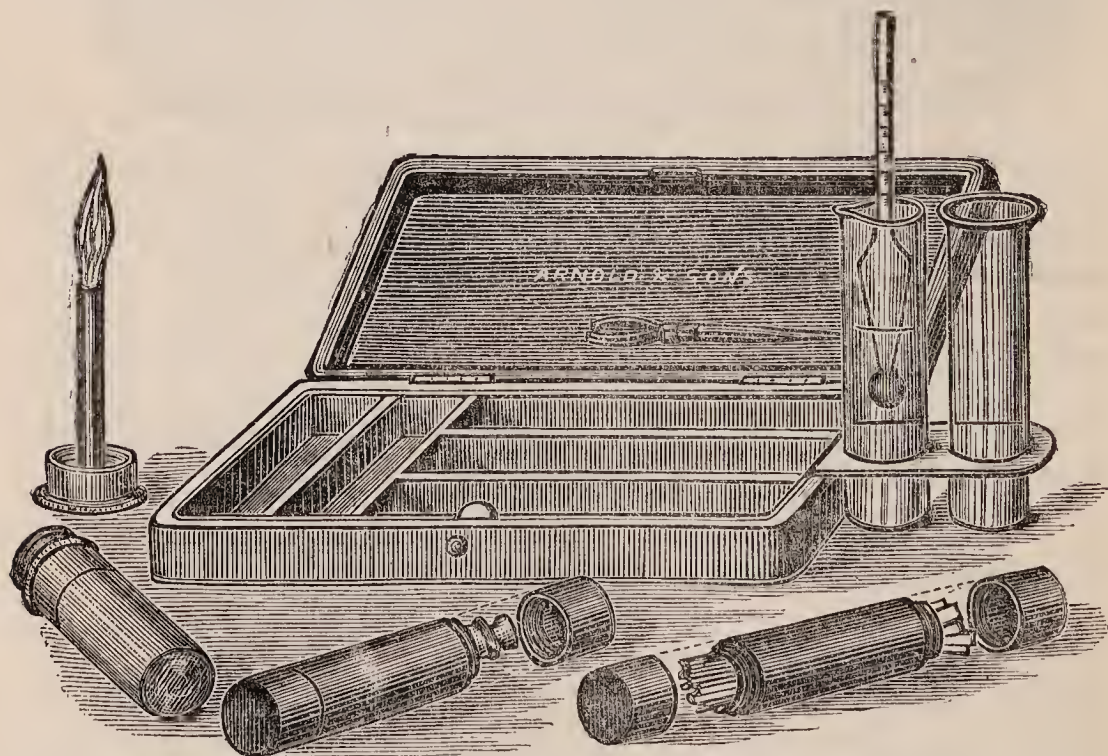
101.—DR. ALEXANDER'S IMPROVED URINARY TEST CASE, FOR USE AT THE BEDSIDE.

This urinary test case has recently been much improved by Messrs. Arnold & Sons, of West Smithfield, London, under direction of its inventor, Dr. R. C. Alexander, Senior Physician to the Bradford Infirmary, and is now as small as possible consistent with strength, durability, and practical utility. It

is $5\frac{1}{2}$ inches long, $2\frac{1}{2}$ wide, and 1 inch deep. It carries a trial jar, enclosing urinometer; a double ebonite case, containing at one end a nitric acid bottle, at the other a bottle for Fehling's test solution or liq. potassæ; two full-sized test tubes; movable bracket for holding trial jar and test tube when in use; a spirit lamp with asbestos indestructible wick; a double ebonite case for test papers and matches; brass forceps for holding test tubes; a glass pipette, &c. The nitric acid capil-



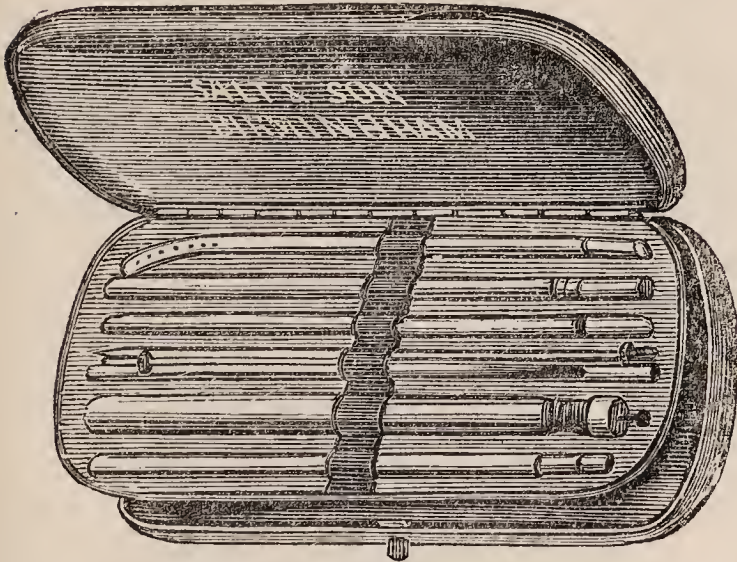
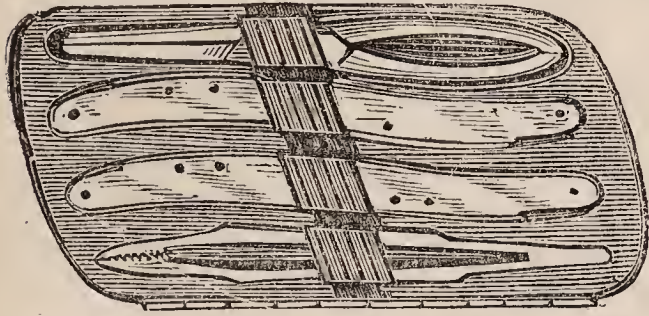
lary tubes having been found unsatisfactory and misleading, a *nitric acid bottle* has been substituted. The case has been



highly spoken of by Sir W. Jenner, Sir W. Gull, Sir H. Thompson, Prof. Paget, Drs. George Johnson, Garrod, Harley, Lionel Beale, Dickinson, Buzzard, Allbutt, and Lauder Brunton.

102.—NEW PORTABLE SURGEON'S DRESSING CASE.

The surgeon's pocket dressing case, of which the accompanying engraving (scale two-thirds size) is a good illustration, is an improvement effected by Messrs. Salt & Son, of Birmingham, on a somewhat similar arrangement recently introduced by them, and described in the *Lancet*. The case is made of aluminium bronze, handsomely engraved, and silk velvet lined,



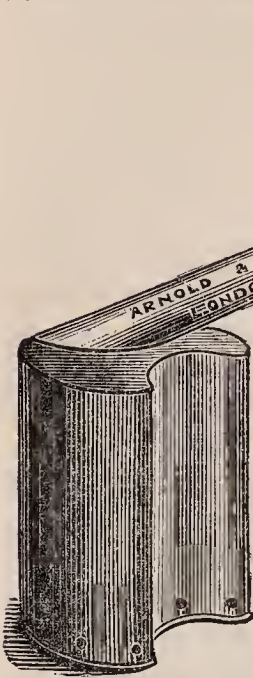
and opens with a hinge like a portemonnaie; a flap is connected with the hinge, and on either side the instruments are arranged and are kept in position by elastic loops. Room is thus provided for four additional instruments, so that the contents of the case are as under: silver caustic case; probes; director; elongating female catheter; thermometer, in case; exploring needle; small skin syringe, for concentrated solutions; folding

scissors; spring forceps; sharp and blunt bistouries; Syme's knife; and tenotome—fourteen instruments in all. The entire case may be conveniently carried in the waistcoat pocket.—*British Medical Journal*, Jan. 18, 1879.

103.—ARNOLD AND SONS' PATENT VAPORIFER.

An apparatus which can be used either as a bronchitis kettle, and inhaler, or a portable vapour-bath, that cannot get out of order or burn, even if allowed to boil dry, that can be refilled without moving from the fire or stopping the generation of steam, is, I think, likely to be useful to the profession, especially at this season. Such an apparatus, which I propose calling the Vaporifer, has been made for me by Messrs. Arnold and Sons, of West Smithfield. The woodcut sufficiently explains its very simple construction. It has no bottom, and when in use stands in any ordinary saucepan, so that, should it boil dry, the

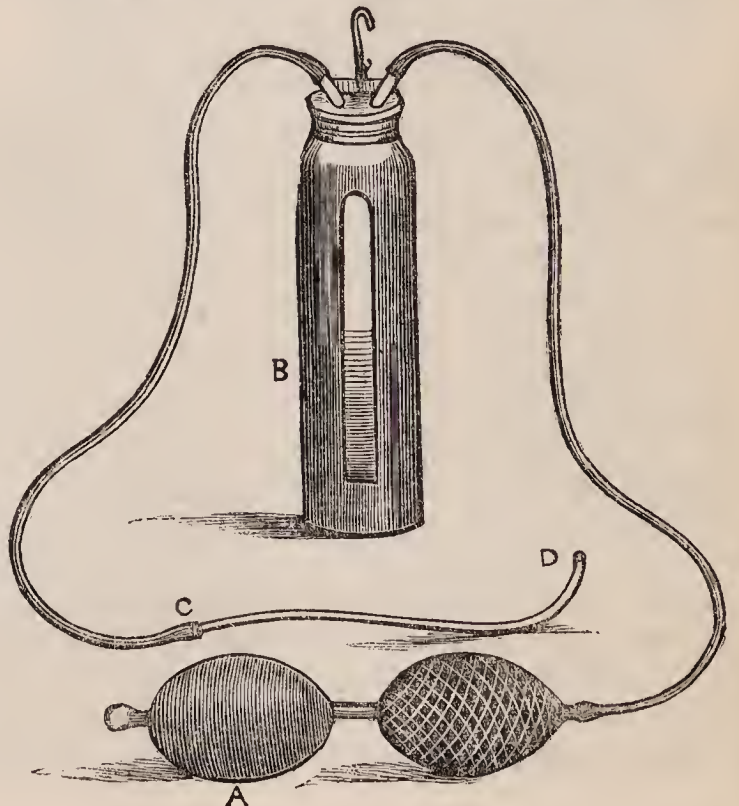
worst that can happen will be the spoiling of a sixpenny saucepan, easily replaced. The top of the apparatus is closed, and on its inner surface, enclosing the orifice of the steam-pipe, is a movable circular chamber, having a perforated bottom, on which any drugs can be placed when it is wished to medicate the vapour. The crescent-shaped front permits of the saucepan being refilled without moving from the fire, and if boiling water be used the steam is generated continuously.



By removing part of the steam-pipe and affixing the mouth-piece, a very handy and efficient inhaler is formed. Placed over a gas-stove or spirit-lamp, it can be used as a portable vapour bath. It can be sold for less than either of the three apparatuses which it replaces, and it packs in a neat, compact case. —Mr. W. L. Shepard, *Lancet*, Jan. 11, 1879, p. 50.

Engraving of Mr. MILLS' Apparatus for Administering Chloroform for Operations about the Mouth, referred to at p. 255.

- A. Hand-bellows.
- B. Receiver.
- C. Elastic tube.
- D. Flexible Metallic tube.



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